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ART. XVI.—*On the Decrease of the Rate of Increase of Population now obtaining in Europe and America;* by HORATIO ROBINSON STORER, of Boston, Professor of Obstetrics and the Diseases of Women in Berkshire Medical College.

(Read before the American Academy of Arts and Sciences, Dec. 14, 1858.)*

IN calling the attention of the Academy to some remarkable and hitherto unexplained facts in the present history of powerful nations, bearing upon their prosperity, progress and even their future existence, I beg heed to the various steps by which I shall arrive at certain astounding conclusions, of the deepest interest to political economists, as well as to moralists.

In many countries of Europe, it has been ascertained that the "fecundity" of the population, in other words the rate of its annual increase, is rapidly diminishing.

* The above paper, as will be seen, was prepared and read nearly ten years ago. It was thought best at the time to confine its discussion to the Medical profession, that any error either in statistics or reasoning might be detected. Portions of it were accordingly published in Philadelphia under the auspices of the American Medical Association, as correlative to the report of a committee, of which the writer was chairman. The event has shown the justice of his conclusions. Not an attempt even has been made to controvert them, while evidence in their favor has been steadily accumulating of an overwhelming character. Physicians are now of one mind as to the existence of the main facts proved. This has been shown by the authoritative issue for general circulation of the late Prize Essay of the Am. Med. Association (Why Not? A Book for Every Woman), and by the publication of corroborative testimony by many practitioners, in different parts of the country. This fact and the increased interest taken in the subject by the leading political economists of Europe, would show that the time was now ripe for its discussion by the scientific world.—H. R. S.

In Sweden, it has lessened by one-ninth in sixty-one years; in Prussia, by a third in 132 years; in Denmark by a quarter in 82 years; in England, by two-sevenths in a century; in Russia, by an eighth in 28 years; in Spain, by a sixth in 30 years; in Germany, by a thirteenth in 17 years; and in France, by a third in 71 years.* Or, to reduce these fractions to decimals—in Sweden the rate of increase has lessened by a fifth, in Prussia by a fourth, in Denmark and England by a third, and in Russia, Spain, Germany and France by a half, in a single century.

For convenience sake, larger bodies of statistics existing concerning it, and from the fact that it represents the extreme of the alleged decrease, I shall take France as the basis of my comparisons.

In France at large, according to the official returns as analyzed by Legoyt,† the increase of the population, which from 1801–06 was at the rate of 1·3 per cent, annually, from 1806–46 had fallen to about ·5 per cent. The exact ratio of decrease after this period is better shown by the figures themselves. The increase from

1841–46	was	1,200,000
1846–51	"	380,000
1851–56	"	256,000

In England during the latter period, with a population of but one half the size, the returns of the Registrar-General showing a relative increase nine times greater.‡

In 37 years from 1817–54 the mean annual increase in France was not more than 156,000; yet in five years, from 1846–51, it had fallen to 76,000 yearly, and from 1851–56 to 51,200; and this, with a population ranging from 29 to 34 millions!

A comparison of these facts with those obtaining in other European states, will make the above still more evident.

TABLE I.—*Rate of increase in Europe* (according to Rau).§

		Per cent.			Per cent.
Hungary (Rohrer),		2·40	Netherlands,	1821–28	1·28
England,	1811–21	1·78	Saxony,	1815–30	1·15
"	1821–31	1·60	Baden (Hennisch),	1820–30	1·13
Prussia,	1816–27	1·54	Bavaria,	1814–28	1·08
"	1820–30	1·37	Naples,	1814–24	0·83
"	1821–31	1·27	France (Mathieu),	1817–27	0·63
Austria (Rohrer),		1·30	" more recently (DeJonnés),		0·55
Scotland,	1821–31	1·30			

A similar and corroborative table, containing additional matter, is given by Quetelet;‖ its differences from the preceding are owing to its representing a different series of years.

* Moreau de Jonnés, Eléments de Statistique, 1856, p. 202.
† Journal des Economistes, March and May, 1847.
‡ Edinb. Rev., Jan. 1857, p. 342; Med. Times and Gazette, May, 1857, p. 462.
§ Lehrbuch der Politischen Oekonomie.
‖ Sur l'Homme et le Développement de ses Facultés, tom. i, ch. 7.

TABLE II.—*Rate of increase in Europe* (according to Quetelet).

	Per cent.		Per cent.
Ireland,	2·45	Austria,	1·30
Hungary,	2·40	Bavaria,	1·08
Spain,	1·66	Netherlands,	0·94
England,	1·65	Naples,	0·83
Rhenish Prussia,	1·33	France,	0·63

And more recently, Legoyt* brings up these results to the close of 1846, by census, and by the annual excess of births over deaths, and is therefore more reliable.

TABLE III.—*Rate of increase in Europe* (according to Legoyt) *by census.*

	Per cent.		Per cent.
England and Scotland,	1·95	Holland,	0·90
Prussia,	1·84	Austria,	0·85
Saxony,	1·45	Sweden,	0·83
Norway,	1·36	France,	0·68
Sardinia,	1·08		

TABLE IV.—*Rate of increase in Europe* (according to Legoyt) *by annual excess of births.*

	Per cent.		Per cent.
Norway,	1·30	Saxony,	0·90
Prussia,	1·18	Hanover,	0·85
Sweden,	1·14	Belgium,	0·76
Holland,	1·03	Bavaria,	0·71
Wurtemberg,	1·00	Russia,	0·61
England and Scotland,	1·00	France,	0·50
Denmark,	0·95	Normandy,	——
Austria,	0·90		

In four departments of France, among which are two of the most thriving of Normandy, the deaths actually exceed the births.†

From the above facts the general mortality not being excessive, it is evident that the percentage of births to the whole population must be smaller in France than in most other European countries; and from the lessened annual rate of increase of the population, that the percentage of births must be decreasing in similar ratio.

From larger statistics furnished by De Jonnés, I have compiled the following table of the comparative ratios of births to the population in the different countries of Europe.

TABLE V.—*Annual ratio of Births in Europe.*

Venice and dependencies 1827,	1 to 23	Sardinia 1820,	1 to 26
Tuscany 1834,	"	Naples and dependencies 1830,	"
Lombardy 1828,	1 to 24	Greece 1828,	"
Russia 1835,	1 to 25	Poland 1830,	1 to 27
Wurtemberg 1821–27,	"	Ireland 1821–31,	"
Prussia 1836,	"	Germany 1828,	"
Mecklenberg 1826,	1 to 26	Switzerland 1828,	"

* Journal des Economistes, May, 1847.

† Mill, Prin. of Pol. Econ., i, p. 343.

TABLE V—continued.

Spain 1826,	1 to 27	Roman States 1836,	1 to 30
Portugal 1815-19,	1 to 27·5	Turkey 1835,	"
Sweden 1825,	1 to 28	Hanover 1835,	1 to 31
Austria 1829,	"	Sicily 1832,	"
Belgium 1836,	"	Austria 1828-30,	1 to 32
Bavaria 1825,	"	Great Britain 1821-31,	"
Two Sicilies 1831,	"	Scotland 1821-31,	1 to 34
Holland 1832,	"	England 1821-31,	1 to 35
Sweden and Norway 1828,	1 to 30	Norway 1832,	"
Denmark 1833,	"	France (1771, 1 to 25) 1851,	1 to 37

In a total population at different periods of 232,673,000, there were 8,733,000 births; whence an average on the grand scale of 1 birth to every 26·6 individuals.

In France, however, the ratio has been steadily lessening; as seen by the following table.

TABLE VI.—*Annual ratio of Births in France.*

1771-75,	1 to 25	1836-40,	1 to 34
1801-10,	1 to 30	1841-45,	1 to 35
1811-25,	1 to 32	1846-50,	1 to 37
1826-36,	1 to 33		

The position of France as compared with the rest of Europe, in respect to the ratio of births to the population at different periods can be made still more manifest.

TABLE VII.—*Comparative ratios of Births in Europe.*

1 to 23,	Venetian Provinces 1827, Tuscany 1834.	1 to 29,	Canton Lucerne 1810, Holland 1832.
1 to 23·5,	Kingdom of Naples 1822-24.	1 to 29·8,	*France* 1801.
1 to 24,	Tuscany 1818, Sicily 1824. Lombardy 1827-28, Russia 1831.	1 to 30,	Sweden and Norway 1828, Belgium 1832, Denmark 1833, Turkey 1835, States of the Church 1836.
1 to 24·5,	Prussia 1825-26.	1 to 31,	Sicily 1832, Hanover 1835.
1 to 25,	*France* 1781, Austria 1827, Russia 1835, Prussia 1836.	1 to 31·4,	*France* 1811.
1 to 26,	Sardinia 1820, Hanover, Wurtemberg and Mecklenberg 1826, Greece 1828, Naples 1830.	1 to 31·6,	*France* 1821.
		1 to 32,	Austria 1830, Great Britain, Switzerland 1831.
1 to 27,	Spain 1826, Germany, Switzerland 1828, Poland 1830, Ireland 1831.	1 to 33,	*France* 1828-31.
		1 to 34,	Norway, Holstein 1826, Scotland 1831, *France* 1834-41.
1 to 27·5,	Portugal 1815-19.	1 to 35,	Denmark 1810, England 1831, Norway 1832.
1 to 28,	Holland 1813-24, Bavaria, Sweden 1825, Austria 1829, Belgium 1836.	1 to 35+,	*France* 1851.

In Paris, strange to say, the decrease in the ratio of births to the population, though decided and steady, has not in actual proportion been as great as in the Empire at large; showing that the cause, whatever it may be, is not one depending on the influence of a metropolis alone for its existence.

From 1817–31 there averaged in Paris 1 birth to 26·87 inhabitants, and from 1846–51, 1 to 31·98.*

* Husson, Les Consummations de Paris, 1856.

The facts thus far stated are admitted by the leading statisticians and political economists of the day, ignorant as they seem of much of the evidence soon to be brought forward, and of the conclusion to which the whole matter directly and with almost mathematical exactness may be proved to tend.

"In France," remarks De Jonnés, "the fecundity of the people is restrained within the strictest limits."*

"The rate of increase of the French population," says Mill, "is the slowest in Europe. The number of births not increasing at all, while the proportion of births to the population is considerably diminishing."†

We turn now to this country, to the commonwealth of Massachusetts.

In the state of Massachusetts, it has been found of late years that the increase of the population, or the excess of the births over the deaths, has been *wholly* of those of *recent foreign origin*.‡ This in 1850, and asserted of the state at large. In 1853, "it is evident that the births within the commonwealth, with the usual increase, have resulted in favor of foreign parents in an increased ratio."§ In other words, it is found that in so far as depends upon the American and native element and in the absence of the existing immigration from abroad, the population of Massachusetts is stationary or decreasing. This is shown also to threaten, even if we allow the foreign element to enter the calculation.

In 1850, the population of Massachusetts was by census 994,665, and the births were 27,664: in 1855 they were 32,845 and the population 1,132,369. The proportion of births to the population was therefore 1 to 36 in 1850, and in 1855 1 to 34; a ratio much smaller than that obtaining in most countries of Europe, and but little over that of France, which in 1850 was 1 to 37.‖

"This result," remarks Dr. Chickering, page 49 of the pamphlet just quoted, "will doubtless surprise many, who will hardly think it possible. Is it general or is it accidental? If it be general, how has it happened? What causes have been in operation to produce it? How is it to be accounted for?" These questions have hitherto been unanswered.

Decrease in the births of a nation, its lessened rate of increase,

* Eléments de Statistique, p. 195.

† Principles of Polit. Economy, i, pp. 343, 344.

‡ Chickering: Comparative View of the Population of Boston. 1850. City Document, No. 60, p. 44.

§ 12th Registration Report to the Legislature of Massachusetts, 1853, p. 116.

‖ The present statistics and others subsequently presented, I have computed from the fourteen published Registration Reports of the State of Massachusetts. Those concerning New York I have drawn from a series of official reports, kindly furnished me by the present City Inspector, Mr. Geo. W. Morton.

must depend, according to one writer, De Jonnés, "either on physical agents, especially climate, or on the degree of civilization of a people, their domestic and social habits." "In France," he again remarks, "the climate is favorable to an increase of population, and this obstacle, this restraint, is found in its advanced civilization."*

"This diminution of births," says Legoyt, "in the presence of a constant increase of the general population and of marriages, can be attributed to nothing else than wise and increased foresight on the part of the parent."†

"The French peasant," writes Mill, "is no simple countryman, no downright '*paysan du Danube*;' both in fact and in fiction he is now '*le rusé paysan*.' That is the stage which he has reached in the progressive development which *the constitution of things has imposed on human intelligence and human emancipation*."‡

"These facts," he again asserts, "are only to be accounted for in two ways. Either the whole number of births which nature admits of and which happen in some circumstances, do not take place; or if they do, a large proportion of those who are born, die. The retardation of increase results either from mortality or prudence; from Mr. Malthus's 'positive,' or from his 'preventive' check; and one or the other of these must and does exist and very powerfully too, in all old societies. Wherever population is not kept down by the prudence of individuals or of the state, it is kept down by starvation or by disease."§

But on the other hand, it has been forgotten by these writers that the alternative supposed does not exist in the case we have instanced. Marriages in France, unlike some other continental states, are continually increasing, and starvation and disease are yearly being shorn of their power.

If we turn to Massachusetts, these arguments acquire additional force. Amid such general thrift, abundance, wealth, in a state comparatively young and not over settled, there has been every reason for the population, general and native, as well as foreign, to increase. Want and excessive mortality are alike absent. Emigration westward and abroad, the only apparent positive check, extensive though this is, can by no means account for the evident facts. Conscription, war, despotism, restraining to a certain extent the population of France, are all unknown to ourselves. With the authors quoted, we are therefore forced to a single position, that this annual lessening of births must be owing, in great measure abroad, almost wholly with us at home, to '*prudence*' on the part of the community, not as a State, which ever encourages population, but as *individuals*.

Before proceeding, I would remark that the condition of things

* Loc. cit., pp. 194, 195.
† Journal des Economistes, 1847.
‡ Loc. cit., i, 336.
§ Ibid., i, 417.

thus far described is such as political economists, almost without exception, approve, and that in great measure it is owing to the direct influence of their doctrines.

In his well known Essay on Population, Mr. Malthus remarks, that "in the average state of a well peopled territory, there cannot well be a worse sign than a large proportion of births, nor a better sign than a small proportion."*

A host of other authorities might be quoted, but a few extracts from a later writer, standard in this country at present and taught in our universities, till very lately in that of Cambridge for instance, will suffice.

"We greatly deprecate," says Mill, "an increase of population as rapid as the increase of production and accumulation."†

"There is room in the world no doubt, and even in old countries, for an immense increase of population. But although it may be innocuous, I confess I see very little reason for desiring it."‡

"I sincerely hope, for the sake of posterity, that they will be content to be stationary long before necessity compels them to it."§

"If the opinion were once generally established among the laboring class, that their welfare required a due regulation of the numbers of their families, only those would exempt themselves from it, who were in the habit of making light of social obligations generally."‖

"The principle contended for includes not only the laboring classes, but all persons, except the few who, being able to give their offspring the means of independent support during the whole of life, do not leave them to swell the competition for employment."¶

"When persons are once married, the idea never seems to enter any one's mind, that having or not having a family, or the number of which it shall consist, is at all amenable to their own control. One would imagine that it was really, as the common phrases have it, God's will and not their own, which decided the number of their offspring."**

"In a place where there is no room left for new establishments," says Sismondi, entirely ignoring the escapes offered by emigration and the increased importation of food, "if a man has eight children, he should believe that unless *six* of them die in infancy, these and three of his own contemporaries, of each sex, will be compelled to abstain from marriage, in consequence of his own imprudence."††

Having now explained an important cause of the effects I have described, I return from the digression.

* Loc. cit., p. 313. † Loc. cit., ii, 253. ‡ Ibid., ii, 316.
§ Ibid., ii, 317. ‖ Ibid., i, 451. ¶ Ibid., i, 452, footnote.
** Ibid., i, 447. †† Nouveaux Principes d'Economie Politique, liv, vii, ch. 5.

Prudence, it is asserted, on the part of individuals checks and keeps within bounds the natural increase of the human race. We cannot well avoid allowing that this statement is true, and that it applies with even more pertinency to ourselves as a people than to nations abroad.

It will be profitable for us to go a step further, and to enquire in what way this result is effected; and though I shall be compelled to refer to matters usually thought best to keep concealed, and to present a conclusion at once frightful, astounding, degrading, I shall not shrink from the duty. For the subject is one which concerns each one of us, as philosophers, parents, as citizens, as christians.

There is no reason to suppose, as West,* Husson and DeJonnés have thought, that the rapid and constant decrease of births I have shown to exist can be attributable to any progressive lack of fecundity on the part of women, or of generative power on that of men; nor is there reason to think that the passions of the race burn less freely than formerly, or that they are more generally under control.

In a certain measure, no greater than formerly however, these needs are met by prostitution. Yet marriages and lawful connections have increased and now undoubtedly exist to a greater proportionate extent than ever before. They are confessed and easily proved, to be usually, either in whole or in great part, barren of offspring—we have only to look about us, for abundant evidence of this—while formerly, as is equally known, such was not the case.

Let all allowances be made for certain conjugal habits, existing extensively among the French, and by no means rarely imitated in this country, as unnatural and degrading as they are detrimental to the physical health of both male and female; but there exist a series of statistics, hitherto unknown, unappreciated or sedulously concealed, which prevent the increasing decrease of births from being thus, and only thus explained.

Prevention of pregnancy, to whatever extent existing, cannot account for the decrease of living births; actual pregnancies being proved fully as frequent as ever. What then can? We answer the question by another.

"Has it been sought," asks Quetelet, in his Theory of Probabilities, though he did not attempt to solve the problem, so puzzling to statistician, philanthropist and statesman, "to account for the peculiarities relating to the still-born, and to combat the causes which in certain circumstances swell their number in so deplorable a manner?"†

I shall show that nearly as many pregnancies exist as ever. We are to consider these pregnancies, not as prevented, but as terminated without the birth of a living child.

* Med. Times and Gazette, June, 1856, p. 611. † Loc. cit., p. 234.

I am aware that the evidence of statistics is received by many minds with a certain measure of doubt; but I shall endeavor so to add proof to proof, and to draw these from such authoritative sources, that no doubt can fairly remain. I base my remarks upon the following self-evident laws.

1st. That, while a result or event in *individual* instances is ever variable and uncertain, this result or event when calculated from or upon *masses* of instances becomes proportionately certain and invariable.

2d. That, to apply this principle to the case we are now considering, the *absolute* number of *living births* in a given population, in a given time, should, in the absence of an evident and sufficient disturbing cause, be always nearly the same; increasing with the increase of the population, and with the progress of medical science (which might easily be proved to be in this respect constantly advancing).

3. That the *absolute* number of *still births at the full period of pregnancy*, occurring from natural causes in a given time in a given population should be always nearly the same; increasing only in proportion to the actual increase of the population, and decreasing with the progress of medical science.

4th. That the *absolute* number of *premature* births, occurring from natural causes in a given time in a given population should be always nearly the same; increasing only in proportion to the actual increase of the population, and decreasing with the progress of medical science.

5th. That the *relative* number of *still births from natural causes*, at the full period of pregnancy and premature, as compared with the *living* births in a given population in a given time should be always nearly the same; *not* being affected by an increase of population, and constantly lessened by the progress of medical science.

6th. That the *relative* number of *still births from natural causes*, at the full period of pregnancy and premature, as compared with the *general mortality* in a given population in a given time, should remain always nearly the same, not being affected by an increase of population and but slightly by the progress of medical science.

7th. That the *relative* number of *still births from natural causes*, premature and at the full period of pregnancy, should remain always nearly the same compared with each other; neither of them being affected by the increase of population and each of them nearly equally by the progress of medical science.

It has already become manifest that the 2d of these propositions does not accord with existing facts; that the *absolute* number of *living* births in Europe and in this country does not remain the same, time and population agreeing; that instead of

increasing with the increase of the latter and with the progress of medical science, it has been rapidly and steadily diminishing.

In the discord of existing facts with the remaining propositions also, I have detected and shall make evident the disturbing cause.

Since 1805, when returns were first made to the Registry of New York, the number, proportionate as well as actual, of fœtal deaths in that city has steadily and rapidly increased. With a population at that time of 76,770, the number of still and premature births was 47; in 1849, with a population estimated at 450,000 the number had swelled to 1320.* Thus while the population had increased only *six* times since 1805, the annual number of still and premature births had multiplied over *twenty-seven* times! The following table shows the rapidity of this increase.

TABLE VIII.—*Ratio of Fœtal Deaths to the population in New York.*

1805, ...	1 to 1633·40	1830, ...	1 to 597·60
1810, ...	1 " 1025·24	1835, ...	1 " 569·88
1815, ...	1 " 986·46	1840, ...	1 " 516·02
1820, ...	1 " 654·52	1845, ...	1 " 384·68
1825, ...	1 " 680·68	1849, ...	1 " 340·90

In the three years preceding 1849, there were registered in New York 400 premature births and 3,139 children still born; a total of 3,539, representing at that time a yearly average of some 1200 fœtal deaths. It is evident that though almost all the still births at the full time, even from criminal causes, are necessarily registered, but a small proportion of the abortions and miscarriages occurring are ever reported.

In the three years preceding 1857, there were registered in New York 1196 premature and 4735 still births, a total of 5931, representing a yearly average of some 2000 fœtal deaths; showing that in the short space of seven years, the number of fœtal deaths in New York, already enormous, had very nearly doubled!

I now present a table showing the ratio of still births to the living births in various countries of Europe.

TABLE IX.—*Ratio of Still to Living Births in Europe.*

Geneva 1824–33, ...	1 to 17	Prussia 1820–34, ...	1 to 29
Berlin (hospitals) 1758–74, ..	1 to 18	Iceland 1817–28, ...	1 to 30
Paris (Maternité) 1816–35, ..	1 to 20	Prague 1820, ...	1 to 30
Sweden 1821–25, ...	1 to 23·5	London (hospitals) 1749–81, ...	1 to 31
Denmark 1825–34, ...	1 to 24	Vienna 1823, ...	1 to 32
Belgium 1841–43, ...	1 to 24·2	Austria 1828, ...	1 to 49

In France at large in 1853 the ratio was 1 to 24. Department of Seine 1 to 15. In the city of Paris 1836–44, 1 to 14·3; in 1845–56, 1 to 13·8. The proportion of still births in the rural districts of France is governed by the same laws as in the metropolis. In 363 provincial towns the ratio was, in 1836–45, 1 to 19·5; in 1846–50, 1 to 18·8.

* Report of the City Inspector for 1849.

While districts more thinly populated gave, in 1841–45, 1 to 29; 1846–50, 1 to 27.*

In Belgium, during a similar period, the ratio was much the same. It was, in 1841–43, in towns 1 to 16·1, in country 1 to 29·4.†

The apparent discrepancy between city and country, noticed as equally obtaining in Belgium and France, is probably owing in great measure to greater negligence of the country officials in registering the still births.

Again, the total number of births at the full time in New York in 1856 was 17,755; of these, 16,199 were living;‡ proving that of children at the full time alone, setting aside the great number of viable children born prematurely, and the innumerable earlier abortions not recorded, 1 in every 11·4 is born dead.

From foreign statistics on a large scale, embodied in the table we have already given, it is found that the proportion of still births does not in those countries drop below 1 in 15, and this in France; ranging from that number up to 1 in 30 or 40 of the whole number of births reported.

In Geneva, out of 10,925 births occurring from 1824–33, 1,221 of them *illegitimate* and therefore to be supposed liable to a large percentage of deaths from criminal causes, there were only 646 fœtal deaths; a proportion of 1 in 17.

In Belgium, there were 29,574 *illegitimate* births from 1841–43, and of these 1,766 were born still,§ or 1 in 16·8.

In New York, from 1854–57, there were 48,323 births; and 5,931 still births, at the full time and prematurely; or in other words, 1 to every 8·1 was born dead.

In Massachusetts, the ratio of still births, at the full time and premature, as compared with the living births in 1850, was 1 to 15·5. In France it is 1 to 24, and in Austria 1 to 49. While the proportion of still births at the full time to the whole number is enormous and steadily increasing, so is the number of known abortions and premature births.

The frequency of these occurrences reported from the practice of physicians, and thus to a certain extent but not entirely, likely to be of natural and accidental origin, is as follows: in 41,699 cases registered by Collins, Beatty, La Chapelle, Churchill and others, there were 530 abortions and miscarriages. Here all the abortions were known; their proportion was 1 to 78·5.

In New York, from 1854–57, there were 48,323 births reported as at the full time and 1,196 premature. Here all the abortions were not known, probably but a very small fraction of them; the proportion was 1 to 40·4.

* De Jonnés, loc. cit., p. 239.
† Quetelet, loc. cit., p. 152.
‡ City Inspector's Report for 1856.
§ Compiled from Quetelet, p. 152.

In Massachusetts, the ratio of premature births to those at the full time, as recorded in the registration reports, during the period from 1850–56, was 1 to 26·1.

That the ratio of still births and abortions, already so frightful, is steadily increasing, is also seen by the following table; in which we have compared the still births, supposable perhaps of accidental value, with the general mortality, whose value is at least as accidental.

TABLE X.—*Ratio of the Fœtal to the general mortality in New York.**

	Total deaths.	Fœtal deaths.	Ratio.
1804–09	13,128	349	1 to 37·6
1809–15	14,011	533	1 to 26·3
1815–25	34,798	1,818	1 to 19·1
1825–35	59,347	3,744	1 to 15·8
1835–55	289,786	21,702	1 to 13·3
1856	21,658	1,943	1 to 11·1

In 1851, the ratio of fœtal deaths in Massachusetts to the general mortality was 1 to 13·3; in 1855, 1 to 10·4, larger than in New York city a year later. In a metropolis we should expect the proportion to be greater than in a state at large; it is here less.

Finally we compare the recorded premature still births of New York, with those still at the full time.

In the seventeen years from 1838–55, there were reported 17,237 still births at the full time, and 2,710 still prematurely; the last bearing the proportion of 1 to 6·3.

In the nine years from 1838–47, omitting 1842 for the reason that the reports to the Registrar for that year were confessedly imperfect, there were 632 still premature births, and 6,445 still at the full time; a yearly average of 1 to 10·2.

In the eight years from 1848–55, there were 2,078 premature still births, and 10,792 still at the full time; an average of 1 to 5; while in 1856, there were 387 still prematurely, and 1,556 at the full time; or 1 to 4·02!

On the other hand, there were recorded in Massachusetts during the 14 years and 8 months preceding 1855, 4,570 still births and 11,716 premature births and abortions,† the ratio being 1 abortion to ·3 still births; or in other words it would appear from the statistics quoted, that the comparative frequency of abortions in Massachusetts is 13 times as great as in the worst statistics of the city of New York!

We are willing however, we rejoice, to modify this statement, as in the earliest of the years quoted, returns from the city of Boston seem to have been imperfect or wanting. We therefore confine ourselves to a more recent period.

From 1850–55, the registration being much more accurate than before, and its results compiled with the greatest care, three

* Compiled from City Inspector's Reports for 1855–6.

† 14th Registration Report, 1855.

years of the five by a noted statistician, Dr. Shurtleff, there were recorded in Massachusetts 2,976 still births and 5,899 premature births and abortions, the ratio being 1 abortion to ·5 still births; in other words, the frequency of abortions as compared with still births at the full time is at least 8 times as great in Massachusetts as in the worst statistics of the city of New York.*

It is allowed by political economists, by Mill and by Malthus himself, that so much of the existing decrease as cannot otherwise be explained, must be attributed to influences generally prevalent in Europe during earlier ages, and in Asia to the present time. "Throughout Europe," says Mill, "these causes have much diminished, but they have nowhere ceased to exist."† Several of these causes, starvation, wars, disease, have been named by the authority now quoted, but the greatest of them all is left unspoken.

The wilful destruction of living children, at and before birth, history declares to have obtained, and to a very great extent, among all the earlier nations of the world, the Jews alone excepted. Aristotle‡ defends it, and Plato.§ It is mentioned by Juvenal,‖ Ovid,¶ Seneca and Cicero; and it is denounced by the early Christians.** It was common in Europe through the middle ages, and still prevails among the Mahometans, Chinese, Japanese, Hindoos, and most of the nations of Africa and Polynesia to such an extent that it may well be doubted whether more have ever perished in those countries by plague, by famine and the sword.

It is impossible that the facts I have quoted from present history can in any great measure be owing to natural causes alone. They are wholly inexplicable on any principles which do not recognize an amount of guilt at which humanity shudders.

We have seen that with us, in the absence of all influences that tend to keep down population in foreign countries, old and crowded, and under the yoke of despotism, the effects attributable elsewhere to these causes, exist and to an *extreme* degree. That the ratio of fœtal deaths to the population had swelled in New York from 1 in 1633 in 1805 to 1 in 340 in 1849, while in France at a later period, 1851, they were only 1 in 1000. That the actual number of fœtal deaths in that city had in the 7 years from 1850–57, very nearly doubled. That the fœtal deaths as compared with the total of births, elsewhere in statistics of illegitimacy alone, where the results are supposed worst and confessed chiefly from crime, being 1 in 16·8 (Belgium), had here,

* The above remarks are not to be misunderstood. In Massachusetts registration has been conducted with greater care than elsewhere. Subsequent investigations have proved that both infanticide and fœticide prevail to an equal extent in many other of our states.

† Loc. cit., i, 417.

‡ Travels of Anacharsis, v, 270.

§ Ibid., iv, 342.

‖ Satires, vi, 592.

¶ Amor., lib. 2; Heroides, epist. 2.

** Reeve's Apologies.

legitimate and natural, reached the frightful ratio of 1 in 8. That the fœtal deaths as compared with the total mortality, had increased from 1 in 37 in 1805, to 1 in 13 in 1855. That the reported early abortions, of which the greater number of course escape registry, bear the ratio to the living births of 1 in 40, while elsewhere they are only 1 in 78. And finally, that early abortions, bearing the proportion to the still births at the full time of 1 in 10 in 1846, had increased to 1 in 4 in 1856.

So far the city of New York—a metropolis, and claiming preëminence neither in morals nor religion. On the other hand in Puritan Massachusetts, in the State at large, and therefore but little affected by the statistics of its capital, which however would by themselves probably be found corroborative of the main result, we have seen that the ratio of still births at the full time and premature as compared with the living births in 1850, was 1 to 15·5. In France it is 1 to 24, and in Austria 1 to 49. That the ratio of premature births to those at the full time, during the period from 1850–56 was 1 to 26, while in New York city it is only 1 to 40. That the ratio of fœtal deaths to the general mortality was 1 to 13 in 1851, and in 1855 1 to 10·4; while in New York city a year later, in 1856, it was only 1 to 11; and that from 1850–55 the frequency of abortions as compared with still births at the full time, was at least eight times as great as in the worst statistics of the city of New York.

Few persons could have believed possible the existence of such frightful statistics, the result toward which they must be confessed inevitably to tend, or the dread cause from which they spring. Either these statistics must be thrown aside as utterly erroneous and worthless, or they must be accepted with their conclusions. We would gladly do the former, but they present too many constant quantities in other respects, as for instance, in the regularly progressive series of deaths and births as compared with the population, constant also as compared with each other, for this to be allowed. My own calculations have been made with care, and I have presented the elements on which they rest. In asserting the results, at once so awful and astounding, I desire to fix upon them the attention and scrutiny of the Academy.

These conclusions however do not rest alone on the statistics that have been presented. The experience of courts of justice, and that equally extensive tribunal, the body of physicians throughout our land, (I regret, and at the same time rejoice, Mr. President, that this assertion is not borne out by your own extended experience,)* tend to corroborate them, and other evidence of equal weight and character is at hand.

* Dr. Jacob Bigelow, then President of the Academy, was inclined at one time to disbelieve in the existence of certain customs everywhere prevalent among us. He subsequently publicly acknowledged however, that his doubts were owing to his not having personally investigated the subject.

In seeking for the *causes* of these facts, I have found much that is interesting, and somewhat that I believe to have been hitherto unpresented.

The immense proportion of living births to the pregnancies in the foreign as compared with the native and protestant population of Massachusetts, already referred to, is to be explained by the watchful protection exercised by the Catholic church over fœtal life. However we may regard the dogma on which this rests, the sanctity of infant baptism, there can be no question that it has saved to the world millions of human lives. But of the various corroborative testimony to which I have alluded, and of other matters pertaining to this subject I shall elsewhere speak.*

Were mankind, in following the advice that has been quoted from past and present authorities in political economy, content merely to practice greater abstinence and greater prudence in sexual matters, less blame could justly be laid. But when we find infanticide and criminal abortion thus justified, rendered common and almost legitimated, we may well oppose to the doctrine of these cruel teachers the words of the indeed admirable Percival, "To extinguish the first spark of life is a crime of the same nature, both against our Maker and society, as to destroy an infant, a child, or a man."†

ART. XVII.—*Research on the Ethers of Silicic Acid;* by C. FRIEDEL and J. M. CRAFTS.

THE determination of the atomic weight of silicium has given rise to more discussion than that of any other element; nor does this astonish us, when we consider the number and complicated nature of the compounds of silicium and the peculiar properties which separate it from all the other elements.

Even at the present day chemists and mineralogists are not agreed whether to write silica SiO_2 or SiO_3, and recently Scheerer‡ has published a paper, in which he brings up the old arguments in favor of the latter formula, and adds to them some new ones, based on the study of the action of silicic acid on carbonate of soda at a red heat, as well as on Wöhler's research on leucon.§ We will not discuss these arguments, as we think that a sufficient reply to them will be found in the facts brought to light by this research, facts which are impossible to reconcile with the opinion of Prof. Scheerer.

* North American Medico-Chirurg. Review; Philadelphia, Jan. 1859, et seq.
† Med. Ethics, p. 79.
‡ Journ. für praktische Chemie, xci, 415.
§ Ann. der Chem. u. Pharm., cxxvii, 257.

It is not our intention to enter into the history of the discussion of the true atomic weight of silicium; but it will perhaps be useful to recall some of its principal phases. In the memorable series of investigations by which Berzelius, without following any fixed rule, but with an accuracy of perception all the more remarkable, established the greater part of the atomic weights, he assigned to silicic acid the formula* SiO_3 (Si=21, O=8). He deduced this result from the analyses of various silicates, particularly of orthoclase, relying upon an analogy which he supposed to exist between silicic and sulphuric acids.

Dumas, when he called the attention of chemists in his classical memoir† to the importance of the determination of the density of vapors and its value in establishing chemical formulæ, gave, among other examples, that of the chlorid of silicium. According to him this body ought to have the formula $SiCl_2$, and consequently silicic acid SiO (Si=7, O=8, Cl=17·75). The chlorids of tin and titanium follow the same law.

Some years later Gaudin,‡ among other remarkable deductions from the law of Ampère, obtained the formulæ $SiCl_4$ and SiO_2 for the chlorid of silicium and for silicic acid. At the same time he noticed the analogy between silicic and carbonic acids. These ideas do not appear to have attracted the attention they merited, because they were too far in advance of those received at the time, and those who have since taken them up have doubtless been unintentionally unjust in not quoting their author.

Ebelmen employed the atomic weight for silicium as established by Dumas, and wrote the formula of silicic ether,

$$SiO, C_4H_5O, (Si=7, O=8, C=6, H=1).$$

This is the most simple expression for the result of his analysis, but his formula is not in accordance with that of Dumas for silicic acid; for in order to correspond with it, silicic ether should be written $SiO, C_4H_{10}O$, as if it contained two atoms of ethyl.§ Indeed Ebelmen's formula implies a hypothesis quite distinct from that, founded on the vapor-density of chlorid of silicium, for, to use the nomenclature of the present day, the first formula represents silicium as monoatomic, and the second as diatomic.

Gmelin‖ wrote the chlorid of silicium and silica $SiCl_2$ and SiO_2 (Si=14, Cl=35·5, O=8), and these formulæ were adopted by the greater number of chemists, and they found a new argu-

* Essai sur la Théorie des proportions Chimiques, p. 134; Paris, 1819.

† Ann. de Chim. et Phys., [2], xxxiii, 367; 1826.

‡ Ann. de Chim. et Phys., [2], lii, 113; 1833. We will add that in this memoir Mr. Gaudin gives the definition of atom and molecule which is received at the present day. And we are happy in being able to render justice to an acute intellect, whose penetration has not been sufficiently recognized.

§ Ann. de Chim. et Phys., [3], xvi, 141; 1846.

‖ Handbuch der Chemie, ii, 339; Heidelberg, 1844.

ment in their favor in the brilliant researches of Marignac on the isomorphism of fluosilicates, fluotitanates, and fluostannates.*

When the progress of organic chemistry forced the chemists, after Gerhardt, to double the atomic weights of oxygen and carbon, leaving that of hydrogen =1, which was nothing else than returning to the old relations of Berzelius and Dumas, the question arose, whether silica ought to be written

$$SiO \text{ (Si=14, O=16) or } SiO_2 \text{ (Si=28).}$$

Gerhardt answered the question implicitly in favor of the latter formula, when he wrote silicic ether† $4(SiO), 4(C_4H_5O)$, and without doubt his only motive for retaining the atomic weight of Ebelmen (Si=7) was the same, which prevented him from changing those of carbon and oxygen in his work on organic chemistry. In order to have been consistent with his own theoretic views, he need only have gone back to the formula of Gaudin. Odling‡ has done this, and writes silica SiO_2, and considers SiH_4O_4 as the normal hydrate of silicic acid, to which the ethers correspond.

Odling, as well as Gerhardt and Gaudin before him, have deduced these formulæ from the consideration of the vapor-density of the chlorid of silicium and of silicic ether. This must be regarded as an important argument in their favor, and to deny its value it would be necessary to forget the admirable order introduced by Gerhardt in the classification of organic compounds in the place of the confusion which reigned before the vapor-density was employed as a criterion to determine the molecular weight of compounds. It would be necessary, also, to ignore the important results obtained by various chemists, particularly by Wurtz§ and Cannizzaro,‖ in the fixation of molecular weights according to the same law.

Nevertheless we must remember that the ideas of Avogadro and Ampère are nothing more than a physical hypothesis; and although this hypothesis has been fruitful in accurate conclusions, even from the stand-point of a chemist, it must yield before purely chemical considerations in the determination of the relative weights of atoms and molecules. This conviction has led us to study the organic compounds of silicium in the hope of obtaining results which would enable us to resolve, on entirely chemical grounds, the question of the true atomic weight of silicium, and we must also admit, with the hope of confirming by our research the conclusions drawn from the law of Ampère.

* Ann. des Mines, [5], xv, 221. † Traité de Chimie Organique, ii, 368.
‡ Philosophical Magazine, xviii, 368.
§ Leçons de Philosophie Chimique; Paris, 1864.
‖ Sunto di un Corso di Filosofia Chimica; Pisa, 1858.

We believe that we have succeeded in demonstrating that the most simple formulæ possible for silicic acid and the normal silicic ether are SiO_2 and $Si, 4(C_2H_5O)$, and in consequence, that the true atomic weight of silicium is 28.

*Silicate of ethyl.**—We chose as point of departure for our research the silicic ether, discovered and studied by Ebelmen. In regard to this body we have little to add to the facts recorded by him. The ether was prepared as recommended by him, taking care to use absolute alcohol, and to add it in small quantities at a time to the chlorid of silicium. When the alcohol is perfectly anhydrous, the quantity of ether obtained is almost equal to the theoretical; however, a small quantity of chlorid of silicium is always carried off by the hydrochloric acid gas which escapes.

Silicic ether, purified by repeated distillation, boils at 165°·5. We determined its density at 0° C. =0·9676. Ebelmen gives the density at 20° C. =0·933. The moisture of the air transforms it rapidly into a solid body, silicic acid; a piece of which, after being kept three years, became hard enough to scratch glass. Notwithstanding its decomposability by moisture, the ether remains unaltered for some time under water, and when it is distilled with water, only traces of silica remain behind in the vessel. These facts must be attributed to its nearly complete insolubility in water, for aqueous alcohol transforms it immediately into a polysilicate, as Ebelmen has already observed. All the water contained in the alcohol does not react immediately upon the ether, for when it is heated during a long time in a closed tube with aqueous alcohol, a larger quantity of polysilicate is obtained than when the two liquids are merely distilled together.

Assigning, like Gerhardt and Odling, the formula $Si, 4(C_2H_5O)$ to silicic ether, we are naturally led to think that it would be possible to replace one-quarter of the ethyl and oxygen (C_2H_5O) by chlorine; as in the diethylic lactic ether an atom of peroxyd of ethyl (C_2H_5O) can be replaced by chlorine with formation of chlorolactate of ethyl.

Diethylic lactic ether. $\left.\begin{matrix} C_3H_4O \\ 2(C_2H_5) \end{matrix}\right\}O_2$

Normal silicate of ethyl. $\left.\begin{matrix} Si \\ 4(C_2H_5) \end{matrix}\right\}O_4$

Chlorolactate of ethyl. $\left.\begin{matrix} C_3H_4O \\ C_2H_5 \\ Cl \end{matrix}\right\}O$

Monochlorhydrine of silicic ether. $\left.\begin{matrix} Si \\ 3(C_2H_5) \\ Cl \end{matrix}\right\}O_3$

In case of silicic ether, we ought to be able to go further, and not only replace one but several atoms of peroxyd of ethyl by chlorine, and obtain the bodies

* The atomic weights used are Si=28, O=16, C=12, H=1.

Dichlorhydrine of silicic ether. Trichlorhydrine of silicic ether.

$$2\left(\begin{matrix}\mathrm{Si}\\ \mathrm{C_2H_5}\\ \mathrm{Cl_2}\end{matrix}\right\}\mathrm{O_2} \qquad \text{and} \qquad \left.\begin{matrix}\mathrm{Si}\\ \mathrm{C_2H_5}\\ \mathrm{Cl_3}\end{matrix}\right\}\mathrm{O}$$

Monochlorhydrine of silicic ether.—This ether was obtained by heating in a sealed tube during one hour at 150° C. three molecules of normal silicic ether with one molecule of chlorid of silicium. The reaction is

$$\mathrm{SiCl_4}+3\left(4\left(\begin{matrix}\mathrm{Si}\\ \mathrm{C_2H_5}\end{matrix}\right\}\mathrm{O_4}\right) = 4\left[3\left(\begin{matrix}\mathrm{Si}\\ \mathrm{C_2H_5}\\ \mathrm{Cl}\end{matrix}\right\}\mathrm{O_3}\right].$$

On distilling the contents of the tube we noticed that there was no free chlorid of silicium remaining. The liquid commenced to boil at 145°, and almost the whole product passed between 153°–160°. After several fractionated distillations, we analyzed the product boiling at 155°·7–157°.

I.	Substance weighed, - -	0·3785 gr.
	CO_2, - - - -	0·5005 "
	H_2O, - - - -	0·2545 "
II.	Substance, weight, - -	1·4545 "
	AgCl, - - - -	1·0650 "
	SiO_2, - - - -	0·4355 "

We will also give here the result of an analysis of monochlorhydrine prepared by the action of chlorid of acetyl on silicic ether, and boiling 155°–157°.

III.	Substance, weight, - -	0·2190 gr.
	CO_2, - - - -	0·2935 "
	H_2O, - - - -	0·1525 "

		I.	II.	III.	Theory.
C	=	36·06		36·55	36·27
H	=	7·46		7·74	7·55
Si	=		13·95		14·10
Cl	=		18·11		17·88

The determination of silicium and chlorine was made by decomposing the body in a flask with an alcoholic solution of ammonia, distilling the alcohol and heating the flask in order to render the silica easy to wash upon a filter. The chlorine was determined in the filtrate from the silica. The alcohol carries over with it on distillation a little NH_4Cl, and it must be carefully condensed and evaporated slowly and the residue added to the filtrate from the silica.

The monochlorhydrine of silicic ether is a colorless liquid, which does not fume in the air, but is rapidly decomposed by moisture or by water with evolution of HCl. It reacts easily on alcohol; HCl is given off and normal silicic ether is formed. It burns with a green flame due to chlorine and with a smoke composed of silicic acid.

Its density at 0° compared with that of water at the same temperature =1·0483. Its vapor density was found to be 7·05. The theory of a condensation to two vols. requires 6·87.

The data for the determination are:

Difference between two weights of bulb,	1·1975 gr.
Temperature of the balance, - -	12°
" " oil bath, - -	230°
Height of barometer, - - - -	762·8 mm.
Capacity of bulb, - - - -	323 cc.
Air remaining in bulb, - - - -	2 cc.

We obtained the same chlorhydrine by heating for about one hour at 170°–180° one molecule of chlorid of acetyl with one molecule of silicic ether. The products of the reaction are acetic ether and the monochlorhydrine, and the reaction may be represented by the equation:

$$4\left(\overset{Si}{\underset{}{}}\right\}(C_2H_5)\Big)O_4 + C_2H_3O,Cl = 3\left(\overset{Si}{\underset{Cl}{}}\right\}(C_2H_5)\Big)O_3 + \left.\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}\right\}O.$$

40 grams of silicic ether, heated with 24 grams of chlorid of acetyl, gave 25 grams of acetic ether, nearly pure, and boiling at 75°–80°. The theory requires 27 grams. Almost the whole of the remainder of the product passed at 155°–158°.

The experiment was undertaken with the object of obtaining an aceto-silicate of ethyl, supposing that the reaction would take place according to the equation:

$$4\left(\overset{Si}{\underset{}{}}\right\}(C_2H_5)\Big)O_4 + C_2H_3O,Cl = 3\left(\overset{Si}{\underset{C_2H_3O}{}}\right\}(C_2H_5)\Big)O_4 + C_2H_5Cl,$$

but no trace of such a body was formed.

The monochlorhydrine of silicic ether is also formed when the ether is heated with the perchlorid of phosphorus; there is formation of a small quantity of oxychlorid of phosphorus and of other more volatile compounds containing phosphorus. A large quantity of chlorid of ethyl is given off. The product obtained in this reaction and boiling at 155°–158° was used to prepare the triethylic mono-amylic silicic ether. See below.

The dichlorhydrine of silicic ether is obtained by a reaction analogous to that which gives monochlorhydrine, by heating together one molecule of silicic ether and one molecule of chlorid of silicium.

$$SiCl_4 + 4\left(\overset{Si}{\underset{}{}}\right\}(C_2H_5)\Big)O_4 = 2\left[2\left(\overset{Si}{\underset{Cl_2}{}}\right\}(C_2H_5)\Big)O_2\right].$$

It can also be prepared by heating monochlorhydrine with the chlorid of silicium.

$$SiCl_4 + 2\left[\begin{matrix}\overset{Si}{\underset{Cl}{3(C_2H_5)}}\end{matrix}\right\}O_3\right] = 3\left[\begin{matrix}\overset{Si}{\underset{Cl_2}{2(C_2H_5)}}\end{matrix}\right\}O_2\right].$$

The reaction takes place somewhat less readily than the one which gives rise to the monochlorhydrine, and it is necessary to heat the sealed tubes longer and to separate the product by a larger number of fractionated distillations. While manipulating the chlorhydrines the utmost care must be taken to prevent exposure to the moisture of the atmosphere, which decomposes them the more readily the more chlorine they contain. The vases containing the different products were always kept in a glass jar, which had the bottom covered with sulphuric acid and a glass cover fitting air-tight.

The dichlorhydrine boils at 136°–138°. Analyses gave:

I.	Substance, weight, - - - -	0·3480 gr.
	CO_2, - - - - - -	0·3270 "
	H_2O, - - - - - -	0·1675 "
II.	Substance, weight, - - - -	0·7005 "
	AgCl, - - - - - -	1·0635 "
III.	Substance, weight, - - - -	0·3250 "
	SiO_2, - - - - - -	0·1000 "

	I.	II.	III.	Theory.
C,	25·62	....	....	25·39
H,	5·35	....	....	5·29
Cl,	....	37·54	....	37·56
Si,	....	....	14·35	14·81

The vapor-density is 6·76; theory 6·545. Data:

Difference of 2 wts. of bulb,	0·8695 gr.	
Temperature of balance, -	22°	
" " oil-bath, -	213°	Air thermometer 211°·5
Height of barometer, -	766·9 mm.	
Capacity of bulb, - -	229·6 cc.	
Air remaining in bulb, -	0·3 cc.	

The density at 0° of the dichlorhydrine is 1·144. This body resembles the monochlorhydrine in all its physical properties.

The trichlorhydrine of silicic ether is obtained by heating for several hours at 150°, either the silicic ether or the two preceding chlorhydrines, with an excess of chlorid of silicium, and separating the products by a large number of fractionated distillations. The precautions that were taken to keep the other chlorhydrines from contact with moisture are still more necessary in the case of the trichlorhydrine. It boils at 103°–105°. Analyses gave:

I.	Substance, weight, - - - -	0·3995 gr.
	CO_2, - - - - - -	0·1976 "
	H_2O, - - - - - -	0·1040 "

II. Substance, weight, - - - - - 0·5945 gr.
AgCl, - - - - - - - 1·4190 "

III. (Boil. point = 104°–106°.) Substance, wt., 0·4235 "
CO_2, - - - - - - - 0·2100 "
H_2O, - - - - - - - 0·1070 "

IV. Substance remaining in the bulb after the determination of the vapor-density, weight, - - - - - - 0·5465 "
SiO_2, - - - - - - - 0·1790 "

	I.	II.	III.	IV.	Theory.
C,	13·46		13·52		13·41
H,	2·89		2·80		2·78
Cl,		59·07			59·33
Si,				15·27	15·58

The vapor-density = 6·378; theory 6·216.

Difference between two weights of bulb, - 0·9317 gr.
Temperature of the balance, - - - 22°·5
" " oil-bath, - - - 163°·5
Barometric pressure, - - - - 766·6 mm.
Capacity of bulb, - - - - - 231·5 cc.
No air remaining.

The density at 0° = 1·291.*

The boiling points of the above compounds, in which a portion of the atoms of the peroxyd of ethyl are replaced by chlorine, represent a series, in which the secondary differences are nearly constant.

Silicate of ethyl, - -	165°·5		
		8°·5	
Monochlorhydrine, - -	157		11°·5
		20	
Dichlorhydrine, - -	137		13
		33	
Trichlorhydrine, - -	104		12
		45	
Chlorid of silicium, -	59		

Triethylic monoamylic silicic ether. — The monochlorhydrine served to prepare this mixed ether, which is nothing else than a normal ethylic silicate, in which one atom of ethyl is replaced by amyl. To obtain it, it is sufficient to bring together a molecule of monochlorhydrine and one of amylic alcohol. The reaction takes place immediately with evolution of HCl.

$$3\left(\begin{matrix}Si\\Cl\end{matrix}\right)(C_2H_5)\Big\}O_3+\left.\begin{matrix}C_5H_{11}\\H\end{matrix}\right\}O=3\left(\begin{matrix}Si\\C_5H_{11}\end{matrix}\right)(C_2H_5)\Big\}O_4+HCl.$$

The liquid commenced to distil at 205°, and almost all passed below 230°. The greater part boiled at 216°–225°. The analysis of different products gave—

* The density of chlorid of silicium at the same temperature we determined = 1·522.

I.	(Boiling point 216°–225°.)	Substance, wt.,	0·2875 gr.
		CO_2, -	0·5560 "
		H_2O, -	0·2620 "
II.	(Boiling point 223°–230°.)	Substance, wt.,	0·7015 "
		SiO_2, -	0·1625 "
III.	(215°–220°.)	Substance, wt.,	0·2215 "
		CO_2, -	0·4280 "
		H_2O, -	0·2070 "
IV.	(215°–220°.)	Substance, wt.,	0·3845 "
		SiO_2, -	0·0940 "

Another preparation where an excess of amylic alcohol had been employed—

V.	(214°–220°·)	Substance, wt.,	0·3795 gr.
		CO_2, -	0·7480 "
		H_2O, -	0·3640 "

	I.	II.	III.	IV.	V.	Theory.
C,	52·74		52·73		53·78	52·80
H,	10·13		10·38		10·75	10·40
Si,		10·80		11·40		11·20

Analysis number V, and those of products boiling above 230°, indicated the presence of a mixed ether, containing more amyl than the one we sought to obtain. This fact awakened our attention, and induced us to make some researches on the action of alcohols upon the ethers of acids, which showed us that an alcohol can act directly upon an ether and that an interchange of the two radicals takes place;* thus when an excess of amylic alcohol is employed, it acts upon the triethylic mono-amylic silicic ether and an ether containing more amyl is formed.

Triethyl monoamyl silicic ether is a colorless, somewhat oily liquid, with a feeble odor like that of all amylic compounds. It is not entirely decomposed by treatment with an alcoholic solution of ammonia, and it is necessary to employ a solution of caustic soda in alcohol to make the determination of silica. Ebelmen made the same observation in regard to the silicate of amyl. Density at 0°=0·926.

The mixed ether on distillation, even in vacuo, has a tendency to decompose with formation of diamylic diethylic ether, and doubtless at the same time of normal ethylic silicic ether. A product which had been prepared from the monochlorhydrine without an excess of amylic alcohol, after several distillations at 100°–110°, under a pressure =3–5 millemeters of mercury, had a composition between that of the mono- and the di-amylic mixed ether.

Substance, weight, - -	0·2805 gr.
CO_2, - - - -	0·5640 "
H_2O, - - - -	0·2750 "

* This Journal, xl, 34.

	I.	$Si,3(C_2H_5),(C_5H_{11})O_4$.	$Si,2(C_2H_5),2(C_5H_{11})O_4$.
C,	54·88	52·80	57·53
H,	10·91	10·40	10·96

Diethylic diamylic silicic ether was prepared in the same way as the preceding by treating the dichlorhydrine with amylic alcohol. The portion boiling at 245°–250° was analyzed.

I. Substance, weight, - - - - - 0·2115 gr.
CO_2, - - - - - - 0·4495 "
H_2O, - - - - - - 0·2095 "

II. Substance, weight, - - - - 0·3980 "
SiO_2, - - - - - - 0·0780 "

	I.	II.	Theory.
C,	57·89		57·53
H,	10·99		10·96
Si,		9·15	9·59

The density at 0°=0·9150.

Monoethylic triamylic silicic ether was obtained in the same way as the preceding with the trichlorhydrine and amylic alcohol. It boils at 280°–285°.

I. Substance, weight, - - - - 0·2410 gr.
CO_2, - - - - - - 0·5350 "
H_2O, - - - - - - 0·2425 "

II. Substance, weight, - - - - 0·4820 "
SiO_2, - - - - - - 0·0875 "

	I.	II.	Theory.	$SiC_{17}H_{38}O_4$.
C,	60·51		61·08	
H,	11·18		11·37	
Si,		8·47	8·38	

This ether resembles in its properties the silicate of amyl. The density at 0°=0·913.

All these mixed ethers can only be prepared from the pure chlorhydrines, as they are decomposed on distillation, and therefore cannot be purified in that way.

Polysilicates of ethyl.—Ebelmen describes, besides the normal silicate, two others, the one containing twice as much silicic acid, and the other four times as much, and calls them bisilicate and quadrisilicate of ethyl.

He obtained the bisilicate, to which he gives the formula

$$2SiO,C_4H_5O \quad (Si=7, C=6, O=8, H=1),$$

by treating the chlorid of silicium with alcohol containing one equivalent (16 per cent) of water. One equivalent of alcohol was added to two equivalents of chlorid.

To quote his memoir, "when the product is distilled, the temperature of the liquid, contained in the retort, rises from 160° to 350° without a considerable quantity of the product distilling.

At this moment the mercury thermometer is taken away, the receiver is changed, and on continuing to heat, soon an abundant colorless product passes. * * * The liquid, distilled above 350°, was rectified with a thermometer placed in the retort; a very small quantity of a product, which distilled below 350°, was collected by itself, then the thermometer was removed, and the distillation continued, and in a few instants almost the whole contents of the retort passed over into the receiver. It did not appear doubtful to me, that a body, which distills with so great rapidity could be other than a definite product, having a fixed boiling point near 350°." According to Ebelmen the bisilicate can also be obtained by adding to the protosilicate alcohol, containing the requisite quantity of water.

As for the quadrisilicate, he says that it is obtained by adding a little aqueous alcohol to the bisilicate, and remains in the retort after the distillation of the latter. It appears to be decomposed into bisilicate and silica at a temperature a little above that at which the bisilicate distills. The existence of these two ethers, if we give them the formulæ

$$2(\mathrm{C_2H_5})\left\{\begin{matrix}\mathrm{Si}\\ \\ \end{matrix}\right\}\Theta_3 \quad \text{or} \quad 4(\mathrm{C_2H_5})\left\{\begin{matrix}\mathrm{2Si}\\ \\ \end{matrix}\right\}\Theta_6 \quad \text{and} \quad 2(\mathrm{C_2H_5})\left\{\begin{matrix}\mathrm{2Si}\\ \\ \end{matrix}\right\}\Theta_5,$$

accords perfectly with what is known of the formation of condensed bodies. Wurtz* has observed the formation of polyethylenic alcohols, which are nothing but the anhydrids of glycol, anhydrids formed by the condensation of several molecules into a single one. He has since 1862† applied to the hydrates of silicic acid the same theory, by means of which he explained the formation of polyethylenic compounds. Wurtz and Friedel‡ have shown that the theory of condensations is applicable to acids as well as to alcohols, and several researches, among others those of Hugo Schiff,§ have confirmed this view. It results from this, that if, in order to take the most simple case, we take the hydrate of silicium, corresponding to the chlorid, in which the 4 atoms of chlorine are replaced by 4 equivalents of the body $H\Theta$,

$$(1.) \qquad \mathrm{Si}\left\{\begin{matrix}\Theta\mathrm{H}\\ \Theta\mathrm{H}\\ \Theta\mathrm{H}\\ \Theta\mathrm{H}\end{matrix}\right. = \mathrm{Si}, 4(\Theta\mathrm{H}).$$

We can derive from this hydrate, which with Odling we call normal, the following hydrates containing less water.

First, without condensation:

* Bulletin de la Société Chimique, [1], i, 82; December, 1859.
† Répertoire de Chimie pure, ii, 449.
‡ Ann. de Chim. et Phys., [3], lxiii, 111; 1861.
§ Comptes Rendus de l'Académie des Sciences, liv, 1075; 1862.

(2.) 1st anhydrid, $Si\left\{\begin{matrix}\Theta H\\ \Theta\\ \Theta H\end{matrix}\right. = Si, \Theta, 2\Theta H.$

The silicate which Odling calls metasilicate corresponds to this hydrate.

Secondly, with condensation:

(3.) 1st anhydrid, $\begin{matrix}Si\left\{\begin{matrix}\Theta H\\ \Theta H\\ \Theta H\end{matrix}\right.\\ \Theta\\ Si\left\{\begin{matrix}\Theta H\\ \Theta H\\ \Theta H\end{matrix}\right.\end{matrix} = 2(Si\Theta), 6(\Theta H).$

This hydrate is the type of Odling's intermediate silicates, bodies which it would be better to call disilicates.

(4.) 2d anhydrid, $\begin{matrix}Si\left\{\begin{matrix}\Theta H\\ \Theta H\\ \Theta H\end{matrix}\right.\\ \Theta\\ Si\left\{\begin{matrix}\Theta\\ \Theta H\end{matrix}\right.\end{matrix} = 2Si, 2\Theta, 4(\Theta H).$

(5.) 3d anhydrid, $\begin{matrix}Si\left\{\begin{matrix}\Theta H\\ \Theta\end{matrix}\right.\\ \Theta\\ Si\left\{\begin{matrix}\Theta\\ \Theta H\end{matrix}\right.\end{matrix} = 2Si, 3\Theta, 2(\Theta H).$

Without giving other formulæ of possible hydrates, we will observe, that the three ethers of Ebelmen would come under the case (1), (2) or (4) and (5). Ebelmen does not speak of any ether corresponding to the formula (3), but this is the only one that we have been able to obtain in a state of purity, and every repetition of Ebelmen's experiment has given negative results, in spite of the most persevering efforts to prepare his bisilicate and quadrisilicate.

Hexethylic disilicic ether.—Having operated upon several pounds of chlorid of silicium, and prepared large quantities of silicate of ethyl with alcohol, which was not perfectly anhydrous, a considerable portion of the product boiled at a higher temperature than the normal silicate. We collected all the products of different preparations, and on submitting them to a fractionated distillation, we observed that a much larger amount of liquid distilled at about 240°, than at any other point above or below between 170° and 320°. In one preparation where 800 grams of chlorid of silicium were employed, we obtained, beside the normal silicate, 80 grams of a product boiling at 230°–240°. An analysis gave:

I. Substance, weight, - - - - 0·3860
$C\Theta_2$, - - - - - - - - 0·5940
$H_2\Theta$, - - - - - - - - 0·3100

Not succeeding well at first in obtaining a compound boiling at a fixed temperature, we submitted all the products, whose boiling point was higher than that of the normal silicate, to a series of distillations in vacuo. This operation can be conducted without difficulty, thanks to the tubes and corks of india-rubber now manufactured,* and the labor of working the air-pump during several days in succession was rendered much less disagreeable by the employment of one of Bianchi's rotary pneumatic machines, with the aid of which we could maintain an atmosphere exercising a pressure equal to only 3–5 millimeters of mercury during all the operations.

After eight fractionated distillations (pressure 3–5 mm.), the product distilling at 125°–130° was analyzed.

II. Substance, weight, - - - - 0·7260
Silica, - - - - - - - 0·2550

III. Substance, wt. (boiling point =126°–130°), 0·3675
$C\Theta_2$, - - - - - - - 0·5690
$H_2\Theta$, - - - - - - - 0·2970

This portion (125°–130° in vacuo), distilled in the air, passed almost entirely at 233°–238°. An analysis of this gave:

IV. Substance, weight (boiling point 233°–238°), 0·1775
$C\Theta_2$, - - - - - - - 0·2745
$H_2\Theta$, - - - - - - - 0·1395

V. Substance, weight, - - - - 0·3750
$Si\Theta_2$, - - - - - - - 0·2005

	I.	II.	III.	IV.	V.	VI.	VII.	Theory. $2Si, 6(C_2H_5), 7\Theta$.
C,	41·96		42·22	42·16				42·11
H,	8·92		8·97	8·74				8·77
Si,		16·38			16·28	16·53	16·59	16·38

All these numbers agree with the formula $\left.\begin{matrix}Si_2\\(C_2H_5)_6\end{matrix}\right\}\Theta_7$, which belongs to one of the ethers of disilicic acid, if this name be given to the hydrate, which results from the condensation of two molecules of the normal hydrate with elimination of the smallest quantity possible of water, i. e., one molecule.

The reaction which gives rise to the ether above analyzed is expressed by the equation

$$2\left[\left.\begin{matrix}Si\\(C_2H_5)_4\end{matrix}\right\}\Theta_4\right]+H_2\Theta=\left.\begin{matrix}Si_2\\(C_2H_5)_6\end{matrix}\right\}\Theta_7+2\left(\begin{matrix}C_2H_5\\H\end{matrix}\Theta\right).$$

* Vulcanized india-rubber becomes less pervious to air after it has been boiled with a weak solution of caustic potash sufficiently long to extract most of the sulphur.

As this ether, which may be named hexethylic disilicic ether, was not obtained by Ebelmen, we have used great care in assuring ourselves, that it is really a distinct compound, and rely as proof upon the following facts. The boiling point after repeated distillations remains constant at a temperature too remote from that of the boiling points, which Ebelmen indicates for the normal ether and for his bisilicate, to make it possible to consider it as a mixture of these two compounds. We succeeded in obtaining the ether almost pure by treating chlorid of silicium with alcohol containing the requisite quantity of water. The product of this experiment was distilled under a pressure of 3–5 mm., and distilled almost entirely at 126°–130°.

This product, boiling at 126°–130° in vacuo, furnished the material for the determinations of silica VI and VII given above.

VI.	Substance, weight, - - - -	0·7305
	SiO_2, - - - - -	0·2585
VII.	Substance, weight, - - - -	0 9370
	SiO_2, - - - - - -	0·3330

We thought at first that the compound was decomposed by heat, and that its boiling point changed after a large number of distillations in the air, and this indeed was the reason, that we undertook the series of distillations in vacuo; but this idea proved afterwards to be erroneous, for a portion, distilled in vacuo after having been heated seven hours at 230°–235° in a sealed tube did not change its boiling point. With silicic ethers as with the chlorhydrines great care must be taken to exclude the moisture of the atmosphere during the protracted operations necessary to effect a large number of fractionated distillations, and if the boiling points changed, it was because the ethers had been decomposed by moisture.

The vapor density of hexethylic disilicic ether corresponds with the formula given above. The first determination was made with an impure product, and the number 13·5 instead of 11·86 was found; but this only came from a portion of an ether having a higher boiling point, which remained in the bulb. This we satisfied ourselves of by making a determination of silicic acid of the liquid remaining in the bulb.

I.	Substance, weight, - - - -	0·8875
	SiO_2, - - - - -	0·3205

Si=16·82 instead of 16·38, the theoretical number. If we suppose that the excess of silica came from an admixture of a body having the composition of the bisilicate of Ebelmen, the liquid remaining in the bulb must have contained one-tenth its weight of that compound.

A second determination made with a product redistilled several times and boiling at 233°–234° gave 12·025, a number which corresponds very closely with the theoretical 11·86.

Difference between the weights of bulb,	1·6595 gr.
Temperature of balance, - - -	12°·5
" " oil-bath (mercury thermom.),	296°
Height of barometer, - - - - -	757·8 mm.
Capacity of bulb, - - - - - -	320 cc.
Air remaining (measured), - - -	26 cc.

11° temperature and 756·8 mm. barometric height.

A determination of silica was made from the liquid remaining in the bulb.

Substance, weight, - -	0·4670
SiO_2, - - - - - -	0·1655

Si=16·48; theory=16·38. The density of the hexethylic disilicic ether is at 0° =1·0196, at 19°·2 =1·0019.

The ether is a colorless liquid of slightly oily consistency, with a rather agreeable odor, scarcely differing from that of the normal silicate. It burns with a smoke composed of silicic acid like all of this class of compounds.

Aqueous alcohol transforms it into products having a higher boiling point. It is not so easily acted on by moisture as the normal silicate.

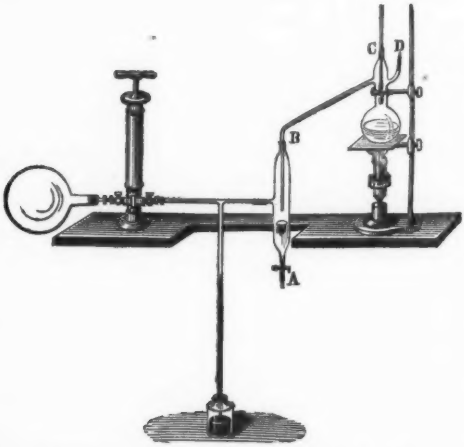
After we had demonstrated the existence of the above body we attempted to obtain the bisilicate of Ebelmen in order to determine whether it corresponds to the first anhydrid of silicic acid or to the second anhydrid of disilicic acid, if this latter exists; but all the experiments undertaken failed to give this body, whose existence seemed so probable on theoretical grounds and whose preparation seemed so easy according to the statements of Ebelmen. We have submitted to a great number of fractionated distillations in the air, under a diminished pressure and in vacuo, the products boiling above 240°, without being able to observe any point, at which a particularly large quantity distilled; and it will be seen by our analyses, that the quantity of silicic acid contained in the higher products, was greater than that required by theory for the bisilicate of Ebelmen. The temperature also, which was measured in vacuo by a mercury thermometer, corresponds to 450° in the air, a point much higher than that at which Ebelmen supposed his compound to boil. It may be of use to give the results of our experiments as they teach us something concerning the nature of the condensed silicates containing more silicic acid than the ether just described.

In order to determine the difference in boiling point due to a diminution of pression, and to ascertain what reliance could be placed on the constancy of boiling points in vacuo, we distilled, under a pressure of 3–5 millimeters, the normal silicate of ethyl having a fixed boiling point at 165°·5 in the air. Platinum wire and a piece of charcoal were put in the flask to make the ebullition as quiet as possible, but without any very marked effect.

The normal ether began to boil at 55° and the thermometer rose rapidly to 72°–73° and remained nearly stationary at this temperature (in another experiment at 74°–75°) during the distillation. When the distillation was interrupted and recommenced, ebullition commenced always at a much lower point than 73°. According to this, the diminution of pressure to 3–5 mm. lowers the boiling point of the ether in question from 165°·5 to about 73°. We have seen that the hexethylic disilicic ether, which distilled at 233°–8°, in the air distilled at 226°–230° under a pressure of 3–5 mm. For this latter product, the limits of temperature within which the thermometer varies during a distillation in vacuo, are much less than for normal ether.

The ebullition is much more regular under a somewhat greater pressure, and for this reason and also because we hoped that the change of pressure might facilitate the separation of the products, as Roscoe has observed in the case of hydrates of acids, we made another series of distillations under a pressure of 60 millimeters of mercury.

These distillations were made with an apparatus which permitted us to operate almost as rapidly as when distilling in the air, and it only requires a small hand pump, when no better is to be procured.



The opening B has an india-rubber tube through which is passed the glass tube leading from the flask containing the substance. At C a thermometer is introduced in the same way. The lower end, A, of the receiver has a tube, which is closed during the distillation, and is opened to draw off the product when required. It is not necessary to surround the receiver

with water, as the specific heat of the silicic ether is very small. The opening D, which is closed with a piece of glass rod fitting into an india-rubber tube, serves to add a new quantity of liquid when required. The object of the other parts of the apparatus is obvious. The balloon at the extremity only serves to augment the volume, so that a constant pressure may be more easily obtained; its communication with the distilling apparatus is closed when it becomes necessary to open the latter. All the connections were made with india-rubber tubes boiled in potash, and used double, the inner one coated with tallow, and when the apparatus was left to itself the pressure only varied a few millimeters in twenty-four hours, and could easily be kept perfectly constant by a few strokes of the air pump during a distillation. We will give the results of several series of distillations made with the same material under different pressures.

[To be concluded.]

ART. XVIII.—*Remarks on the Cretaceous rocks of the West known as No.* 1, *or the Dakota Group;* by F. V. HAYDEN.

THE Cretaceous rocks of the Upper Missouri have been separated into five divisions, which have been designated, for the sake of convenience, by Nos. 1, 2, 3, 4, and 5.* They have also received special geographical names indicating points where they are shown in their largest development.† The Dakota group or Formation No. 1, lies at the base of this series, and either as an outcropping or underlying rock extends over a large portion of Nebraska, Dakota, Minnesota, Kansas, and even reaches far southward into New Mexico. It is more distinctively defined along the Missouri river between Omaha City and the Big Sioux, where it exhibits its typical characters.

The lithological characters of this group have been so often described in former memoirs by Mr. Meek and the writer, that they scarcely need to be repeated here. The principal object of this article is to present such additions to our knowledge of it as may have been obtained since our last papers were published. It is not intended to discuss in detail any differences of opinion, or to make any criticisms, but to present certain facts and statements gathered by Professors Capellini and Marcou in their tour to Nebraska in 1863, and the results of the investigations of Prof. Heer derived from the study of the flora of this group.

* Memoirs Am. Acad. Arts and Sci., vol. v, new series, also numerous papers by F. B. Meek and F. V. Hayden.

† Proceedings Acad. Nat. Sci., Dec. 1861.

In order that the geological relations of the Dakota group may be better understood, I have given a brief summary of the contiguous formations.

In ascending the Missouri river we find near Fort Leavenworth exposures of limestone which, up to the present time, we have regarded as belonging to the upper Coal-measures. The beds are all nearly horizontal, with a slight, almost imperceptible, dip toward the northwest. As we continue up the Missouri, layer after layer of these Coal-measure rocks pass from view beneath the water level of the river, and when we reach Fort Lisa they have entirely passed from sight and are overlapped by a bed of variegated friable sandstone. The sandstone undoubtedly exists, or has existed, as an underlying rock considerably lower down the river than Fort Lisa, probably nearly to Omaha City, but has either been removed by erosion, or concealed by the great thickness of superficial recent deposits which cover this country, sometimes entirely hiding from view the underlying basis rocks over large areas. Ascending the Platte valley we have the true Upper Carboniferous limestone nearly to the mouth of the Elkhorn. Before reaching that point, however, we observe a portion of No. 1 resting directly upon the limestones, as the following section will show.

1. Gray compact siliceous rock, passing down into a coarse conglomerate, an aggregation of water-worn pebbles, cemented with angular grains of quartz; then a coarse grained micaceous sandstone (lower portion of Dakota group), 25 feet.

2. Yellow and light gray limestone of the upper coal-measures containing numerous fossils, *Spirifer cameratus*, *Spirigera subtilita*, *Fusulina cylindrica*, *Productus*, *Chonetes*, and abundant coral and crinoidal remains.*

At the mouth of the Elkhorn, the Carboniferous limestones have passed from view beneath the Cretaceous sandstones. The intermediate Permo-carboniferous and Permian rocks, as well as the variegated and gypsiferous marls and clays which are quite conspicuous westward from Fort Riley, are wanting in this region. The Dakota group as seen along the Missouri passes beneath the bed of the river about 30 miles above the mouth of the Big Sioux. How far up the Big Sioux it extends is not yet known; but at a point 40 miles up the valley, the seam of earthy Lignite, which is seen just above the Omaha reserve, crops out, and is exciting some attention among the farmers.

The researches of Prof. Hall have extended this group northward from the Missouri river into Minnesota 130 miles or more. In an interesting memoir read before the American Philosophical Society at Philadelphia, Prof. Hall, after describing numerous exposures of the variegated quartzites from St. Peters to Fort

* Trans. Amer. Phil. Soc. 1861.

Ridgely, says: "Proceeding westward from Fort Ridgely, I had no opportunity of seeing any other formation than the prairie for about thirty miles. At this point near where we crossed the Big Cottonwood River, there is an exposure of rock in the bank of the stream; and at a short distance farther on, some explorations had been made for coal, and a shaft had been sunk to the depth of more than one hundred feet. The materials thrown out of this shaft consisted of a dull greenish argillaceous sand, with calcareous nodules, together with irregularly laminated sandstone containing vegetable remains. The order of deposits, as given to me by Mr. Morin, who superintended a part of the working, was as follows:

1. Ironstone; 1 ft. 6 inches.
2. Sand, clay, etc.; 40 ft.
3. Earthy coal;* 1 ft. 8 inches.
4. Sand, clay, etc.; 3 ft.
5. Sandstone in irregular and diagonally laminated layers, with sometimes calcareous concretions, and containing plant remains; 5 ft.
6. A Calcareous sandy clay of variable color and character; 20 ft.
7. Sandstone in loose thin layers of three or four inches; 4 ft.
8. Clay with coaly seams near the bottom; 16 ft.
9. Clay; 13 ft.
10. Loose quicksand to bottom of shaft.

"In the river bank, at a quarter of a mile distant, and at a level 30 or 40 feet below the ground where the shaft began, there is the following exposure:

1. Loose ironstone† in nodules and irregular concretions, more or less mixed with drift and pebbles; 1–2 ft.
2. Calcareous clay; 6–8 ft.
3. Earthy coal; 8 inches.
4. Clay as above coal; 4 inches.
5. Yellow or ferruginous sand and clay; 3–3½ ft.
6. Ferruginous sandstone in irregular layers and diagonally laminated to level of river; thickness unknown.

"This sandstone appears to be the same as that containing the vegetable remains met with in the shaft; and though I did not find plants in it at this point, I was informed that specimens had been found there; and at another place on the Cottonwood I found them to be quite common. Near the previous exposure,

* An analysis of this coal, by Prof. T. Sterry Hunt, gave the following results:

Fixed carbon, ...	26·1
Volatile "	25·7
Ash "	48·2=100·0

In the sections of strata near the mouth of the Redwood river, there is a stratum of similar earthy coal three feet thick.

† This ferruginous layer does not appear to belong to the regularly stratified deposits, as it overlies, irregularly, the edges of the successive beds, and has been deposited after the denudation had taken place.

and partly from an old digging, I obtained a similar section. At still another point I noticed a similar exposure, leaving no doubt of the character and order of arrangement of the materials composing this formation.

"The character of the vegetation obtained from the sandstone of the shaft and elsewhere, resembling the leaves of Salix, Poplar, Liriodendron, Tupelo, etc., induced me to refer this formation to the Cretaceous period. A single indistinct shell was the only animal fossil I was able to obtain. In aspect the calcareous concretions are similar to those from the Cretaceous formation of the upper Missouri; and the green argillaceous clay is likewise similar."

At Redwood Falls, also, Prof. Hall obtained another section of this group with a seam of earthy coal three feet in thickness. There is little doubt that this group will be found extending far northward, either as a continuous formation or in isolated patches, perhaps even into the British possessions.

In September, 1863, Mr. Marcou and Prof. Capellini of the University of Bologna, Italy, made an excursion up the Missouri river to a point near the mouth of the Big Sioux. It seems that both of them studied with care the different geological formations along the river under quite favorable circumstances. Prof. Capellini made quite an extensive collection of fossil plants at different localities, principally from Tekama, Blackbird Hill and Big Sioux. These plants he placed in the hands of Prof. O. Heer of the University of Zurich, Switzerland, who described them in a small but carefully prepared memoir with four plates, 4to. Mr. Marcou also published the results of his examinations in the Bulletin of the Geological Society of France. He there acknowledges that the rocks, which have hitherto been included in No. 1 or Dakota group are Cretaceous. Capellini and Heer came to the same conclusion, and thus one disputed point in regard to the geology of the West may be regarded as forever set at rest. I would remark just here, that I have personally examined the greater part of the territories of Kansas, Nebraska, Dakota, Montana, Idaho, and Colorado, and I feel confident that, although future investigations may perhaps modify, they will not essentially change, the published results.

After examining the Carboniferous limestones as far as Bellevue, Nebraska, Mr. Marcou* continued up the Missouri and examined the sandstones of the Dakota group. In this red sandstone he found a rich flora of well preserved leaves of Laurel, Poplar, Sassafras, Walnut, Oak, Willow, Tulip, a flora which

* Reconnaissance géologique au Nebraska, par Jules Marcou: Extrait du Bulletin de la Société Géologique de France, 2d série, t. xxi, p. 132, Jan. 18th, 1864, and Les Phyllites Crétacees du Nebraska, par MM. les Prof. J. Capellini et O. Heer, tirage a part des Mémoires de la Société Helvétique des Sciences Naturelles.

Prof. Heer regards as Miocene, and has more the aspect of upper Miocene or even Pliocene than of the lower Tertiary epoch. Nevertheless, the flora is not even Tertiary but really lower Cretaceous. It is found in a fresh-water formation at the bottom of the White Chalk of the Missouri Basin. It was Dr. Hayden who first collected these dicotyledonous plants at Blackbird Hill, at the Omaha Mission, where a quarry was opened for the purpose of erecting the mission house. Mr. Heer who saw the designs of the first collection made at Blackbird Hill by Dr. Hayden, declares that this flora is not Cretaceous, but that on the contrary it has a very close analogy with the lower Miocene or Oligocene of Europe. But notwithstanding these facts, Mr. Marcou saw superimposed upon the rocks with dicotyledonous leaves at Pilgrim Hill and on the banks of the Big Sioux, beds of chalk containing *Inoceramus problematicus*, *Ostrea congesta*, &c., and that too without any indications of faults or disturbances of strata. "I yield to the opinion of Mr. Hayden and regard these beds as Cretaceous. But I make one reservation; that all the Cretaceous beds of Nebraska, and of the upper Missouri in general, are very recent and correspond, as also those of New Jersey, to the Senonien of d'Orbigny, and perhaps also to his Turonien."

From his observations in Nebraska, Mr. Marcou arrives at the following conclusions.

1. "The rules and laws of paleophytology hitherto adopted and followed must be greatly modified, since we find here a flora, regarded as Miocene in Europe, at the base of the Chalk period. On this account I may be permitted to add that, in the geographical distribution of the existing flora, I have discovered contrasts much greater than those in the distribution of the existing fauna. One of these contrasts which surprised me most is that of the elevated plateaus of New Mexico, Texas, Arizona and California, which is certainly more different from the flora of the States bordering upon the Atlantic and the basin of the Mississippi under the same conditions of temperature and latitude, while on the other hand it does not differ from the Tropical flora of Florida, the Antilles and Panama. After this discovery I could see no serious objection which would have weight in the mixture of Carboniferous plants and Belemnites at Petit-Couer in Laventaire, since in Nebraska we have Miocene plants underneath 500 or 600 feet of white chalk containing *Inoceramus*, *Ammonites*, and *Baculites*.

2. "The new red sandstone and more especially the lower portion or dyas occupies a very important place in the geology of Nebraska, as I announced it in 1855 to the Geological Society of France, in my geological chart of the United States and in the explanatory résumé which accompanied it.

3. "The dyas of Nebraska is composed of two members, as in Russia and Germany, one of which corresponds to the Rothliegende and the other to the Zechstein."

Prof. Capellini in a short but very interesting article confines his observations mostly to the rocks of the Dakota group and remarks that he does not hesitate to regard the observations of American geologists as entirely just. The following remarks close the article of Prof. C.

"After all we have observed in relation to the environs of Sioux City, it is easily seen that a stratigraphic series so complete throws a clear light upon the isolated facts first noticed at Tekamah and Blackbird Hill, and indicates the exact position of the rocks with dicotyledonous leaves, analogous to the Tertiary leaves of Europe but belonging in reality to the Chalk.

"It may be estimated that the thickness of these Cretaceous strata in the environs of Sioux City is about 40 meters. They may be divided into two distinct parts, one rich in leaves, a fresh-water formation; the other truly chalky with fishes and Inoceramus, of marine origin—both are probably not older than the chalk of Maestricht. This has been my opinion from the time I admitted that the dicotyledonous leaves of the Big Sioux and Tekamah were Cretaceous.

"Once the age of the Mollasse with leaves established by the aid of the stratigraphy and the animal fossils, it would be interesting if it were possible to arrive at the same results by the vegetable remains. On this account, Prof. Heer came to my aid and investigated the specimens I collected in my explorations. More than a dozen species were recognized among the leaves from Tekamah, Blackbird Hill, and Big Sioux, but it was especially the first locality which furnished the best specimens. We are convinced that when observations are exact and determinations made from careful examination of specimens, there is never any disagreement between stratigraphical and paleontological laws."

The remarks of Prof. Heer which preface his descriptions of the fossil plants collected by Prof. Capellini, are so interesting and important that we copy them entire.

"The collection of Mr. Capellini contains 16 species; 4 are badly preserved; 12 are determinable. Nevertheless, of the latter, several are but fragments, so that their determination is difficult and not sufficiently positive. This is especially the case with the Phyllites which I have referred to the genera Platanus and Andromeda. It is certain that all the leaves found by Mr. Capellini are dicotyledons and with great probability one may be referred to the genus Ficus, one to Salix, one to Diospyrus, two to Populus and two to Magnolia, although there are no accompanying fruits or other parts to confirm these determina-

tions. These genera are yet living and they are also found in the Tertiary formations.

"If we compare these plants of Nebraska with the Cretaceous plants of Europe, we find no identical species among them. I sent drawings of them to Dr. Debey of Aix la Chapelle, who discovered in that locality a Cretaceous flora. He has written to me that he has not found one species identical. Even the greater part of the genera are different. There is but one Cissites (*C. aceroides* Debey) which recalls slightly the *C. insignis* (Plate IV, fig. 5). The Cretaceous plants of Henant, Belgium, those of Blankenburg and Quedlinburg are also very different.

"Prof. Schenk has recently sent to me, a collection of plants of Quedlinburg for determination. Besides conifers and ferns characteristic of the chalk, it contains dicotyledons, but no forms like those of Nebraska.

"The Cretaceous flora of Moletein, Moravia, which I have lately studied, exhibits more resemblance. It contains two species of Ficus which much resemble the Ficus of Nebraska, two superb species of Magnolia, one with a fruit cone. There is a relationship between the flora of Nebraska and that of the upper Chalk of Europe, although identical species are wanting. But to the present time no characteristic genus of the Cretaceous flora of Europe has been found in Nebraska.

"If we compare the plants of Nebraska with the Tertiary plants we find no identical species, but 7 genera, (Populus, Salix, Ficus, Platanus, Andromeda, Diospyrus and Magnolia) are also Miocene and likewise living. It then appears that the Nebraska flora is related more to the Tertiary than to the Cretaceous flora of Europe, a fact which struck me when I first saw drawings of the former. But it should be remarked that we know but a very small number of American species, and on the other hand, the European Cretaceous flora has more relationship with the Tertiary flora than I at first supposed. I have found in the Cretaceous flora of Moletein, Moravia, species of Ficus and Magnolia which resemble Tertiary species; a Myrtacea, which is a near neighbor to the *Eucalyptus rhododendroides*, Mass, of Mt. Bolca; a Juglans and a Laurinea, which also have their analogues in the Tertiary flora; a Pinus and two other conifers which belong to the genus Sequoia, which was extensively distributed in Europe and America in the Miocene epoch and which is now only found in California.

"As the Cretaceous fishes are more nearly related to the Tertiary than to the Jurassic fishes, the upper Cretaceous flora is also entirely different from the Jurassic and more nearly allied to the Tertiary floras, and it appears that in America the relation between the Tertiary and Cretaceous flora is yet more intimate than in Europe.

"It is remarkable that the plants of Nebraska (as Magnolia and Liriodendron) present relations with the existing flora of America, whilst the Cretaceous flora of Europe has more of an Indo-Australian character. It thus appears that since the Cretaceous epoch, the American flora has not undergone a change so great as the European flora. While the Cretaceous flora of Europe is entirely different from the existing European flora, that of Nebraska contains 8 genera yet found in America, and it is the more remarkable that the greater part are yet found in a country under the same latitude."

Prof. Heer describes the following species of plants from this group in this memoir: *Populus litigiosa, P.? Debeyana, Salix nervillosa, Betulites denticulata, Ficus primordialis, Platanus? Newberryana, Proteoides grevilleæformis, P. daphnogenoides, P. acuta, Aristolochites dentata, Andromeda Parlatorii, Diospyrus primæva, Cissites insignis, Magnolia alternans, M. Capellini, Liriodendron Meekii, Phyllites Vanonæ.*

Both Mr. Marcou and Prof. Capellini agree in regarding this sandstone in which the dicotyledonous leaves are found as a fresh-water formation. I would simply say that I have always regarded it as marine and I am sure this has been the opinion of my friend Mr. Meek. At any rate we have found mingled with the leaves at Sioux City quite well preserved casts of *Pharella? Dakotaensis, Axinœa Siouxensis* and *Cyprina arenacea*, shells peculiar to marine deposits.

The question has arisen, whether this period had a fauna corresponding to its flora? Besides the Mollusca already alluded to, no remains of land animals have yet been discovered which could be positively identified. On the eastern slope of the Big Horn mountains, I saw in 1859, a series of beds holding a position between No. 2 Cretaceous and the Jurassic rocks below, which I referred without hesitation to this group. In these rocks were beds of earthy Lignite, large quantities of petrified wood, and numerous large uncharacteristic bones which doubtless belong to some Saurian. No remains of strictly land animals have ever been found.

Although I have endeavored to give the substance of Mr. Marcou's memoir in this article, I do not wish to be regarded as endorsing all that he has said in regard to the existence of "Dyas" and "Mountain limestone." In the autumn of 1856, while descending the Missouri river, I saw some peculiar local beds near St. Stephen's, Nebraska, from which I obtained *Myalina perattenuata, Pleurophorus occidentalis* and *Lyonsia concava*, which were sufficient to indicate that they were either Permian or the transition beds which we have called Permo-carboniferous. These questions have been most ably and exhaustively discussed by Mr. F. B. Meek in an article in this Journal, Jan., 1866, to

which the reader is referred. The main object of the present article is to place before those American readers of this Journal, who may not have access to foreign Journals, such views and important geological facts as Profs. Marcou and Capellini obtained in their field investigations, as well as those of Prof. Heer from the study of the fossil plants.

University of Pennsylvania, Philadelphia, Jan. 8th, 1867.

ART. XIX.—*Researches on Solar Physics*;* by WARREN DE LA RUE, Esq., Pres. R.A.S., BALFOUR STEWART, Esq., Superintendent of the Kew Observatory, and BENJAMIN LOEWY, Esq., Observer and Computer to the Kew Observatory.

First Series.—On the Nature of Sun-spots.

1. THERE is a marked difference between our luminary and our satellite, as far as regards our knowledge of their physical aspect and constitution. Many parts of our own globe are not so well known, or so correctly mapped, as certain regions in the moon; and could we imagine an observer transported into the neighborhood of Tycho or Copernicus, he would probably be better prepared for the appearance presented to him, than he would be if placed suddenly in equatorial Africa or central Australia. But with regard to the sun the case is very different; for although the progress of science has enabled us to detect the presence of certain familiar substances in the atmosphere of our luminary, it has hitherto only shrouded in deeper mystery than ever the origin of that wonderful outpouring of light and heat which is the sun's most prominent characteristic, and to this very day it has not been finally decided whether this luminosity proceeds from the sun's solid body, or from an envelope which surrounds it. Indeed so strange and so unaccountable are many of the features presented to us, not only by our own sun, but by many of the stars, that it has even been conjectured that these bodies exhibit instances of the operation of some force of the nature of which we are yet ignorant. If we accept this view of the case, the study of our luminary becomes one of very great importance, but one in which we must be very careful to be guided by observation alone. We must obtain numerous and accurate representations of the sun's surface, and study these carefully and minutely, before we attempt to generalize.

§ I. *Methods of observation.*

2. There are two methods of accomplishing this. (1.) Eye-observations of the sun's surface may be made by means of a

* From a memoir printed for private distribution.

telescope, and the appearance carefully mapped by the observer. (2.) Or we may call to our aid that art which has already proved of signal service in many branches of science, and, by means of photography, obtain autographs of our luminary, which we may measure and examine carefully at our leisure.

Each of these has its advocates, but it is not our design to discuss the comparative merits of the two methods; on the contrary, as each has its own special advantages, we are willing to adopt them both, and to avail ourselves of all those materials which our own observations or the kindness of friends may have put into our hands.

§ II. *Historical sketch.*

3. The most important knowledge which we possess regarding the physical appearance and structure of our luminary is derived from the following sources.

4. *Sun's rotation.*—We are, in the first place, indebted to Galileo, if not for the first discovery of sun-spots, at least for the first attempt to ascertain through their means the period of rotation of our luminary.

5. *Nature of sun-spots.*—The next great advance in solar physics is due to Alexander Wilson, Professor of Astronomy at Glasgow, who in 1773, communicated a paper to the Royal Society, describing certain phenomena with regard to spots, which, in his opinion and in that of many others, appear to indicate that spots are cavities in a luminous photosphere which surrounds the sun.

The accuracy of this conclusion has recently been questioned; but whatever may be said regarding the theory, there can be no doubt regarding the importance of the fact which was first revealed by Wilson.

6. *Their periodicity.*—The next step is due to Hofrath Schwabe, of Dessau, who has shown, as the result of nearly forty years' laborious observations, that the number of spots which break out on the sun's surface is not the same from year to year, but has a maximum about every ten years—a remark which led General Sabine to observe that the various epochs of maximum spot-frequency were also those of maximum magnetic disturbance in our own globe.

7. *Their proper motion, &c.*—Carrington is the next observer who has greatly extended our knowledge of this subject. In a large and most remarkable work recently published, and containing the result of many years' observation, he has shown that sun-spots have a proper motion of their own, those near the solar equator moving faster than those near the poles; and he has also made interesting remarks on the distribution of spots in solar latitude for different years. In addition to these new

facts, he has furnished us with very accurate data regarding the sun's rotation.

8. *Gradations in their luminosity.*—We ought also to mention the discovery by Dawes, that what is regarded as the umbra of a spot consists in many cases of two well-defined and separate parts,* the exterior part being less luminous than the interior; so that we have often connected with the same phenomenon not less than five degrees of luminosity: these are—(1.) The faculæ. (2.) The ordinary photosphere. (3.) The penumbra. (4.) The borders of the umbra. (5.) The very dark central nucleus.

Mr. Dawes's discoveries are mainly due to his employing, with an eye-piece of his own invention, the full aperture of the telescope; but it is necessary to recall the fact that Sir William Herschel, in earlier times, was fully aware of the importance of not contracting the aperture of the objective. Moreover, we must not forget that Sir W. Herschel contributed to solar physics a theory which still holds its ground.

9. *Red flames.*—But there is another phenomenon connected with our luminary, not less curious than solar spots. We allude to the red flames, or protuberances which are seen to surround the sun's disk on the occasion of a total eclipse. Airy and Arago were the first to conjecture that these belonged to the sun. In the total eclipse of 1851, the former of these observers, by combining his observations with those of O. Struve, showed it to be probable that these flames do not change *during the moon's motion.* Great credit is also due to this observer for organizing the Spanish expedition of 1860; and it was here that Mr. De la Rue, by means of the Kew heliograph, set the matter completely at rest. Mr. De la Rue, from the pictures which he obtained, was able to show that the flames only change apparently, not really, by the moon's motion over them, that is, by covering one portion and disclosing another, and do not otherwise undergo any alteration; so that when the clock of his instrument was adjusted to the sun's motion, that portion of the flames not covered by the moon stood still. He also showed that the angular motion of the red flames, with respect to the moon, corresponds to the theory of their fixation in the sun.

These results were verified by Secchi, who also obtained photographs of the same phenomenon, which were compared in Rome by Mr. De la Rue and Father Secchi with Mr. De la Rue's photographs. The forms of the red prominences were found identical in both, so that no change occurs in their form during an interval much longer than the duration of totality observations in a solar eclipse.

* In some cases, however, it is fair to assume that the appearance of lighter portions of the umbra may be caused by the floating across of portions of the brighter part of the sun's surface.

10. *Willow-leaves.*—We may be allowed to mention here that very lately Mr. James Nasmyth, during the course of his observations of the sun's surface, has come to the conclusion that, when the circumstances of observation are very favorable, the whole surface will be found to be composed of separate luminous bodies, of a great similarity of figure, interlacing one another; and he has given the name of *willow-leaves* to these appearances. The existence of these is still disputed; but some of our best observers in this country have seen them under very favorable atmospheric conditions, and they have been seen more frequently by Secchi and other Italian observers.

11. *Other observations of the sun's surface.*—Chacornac, the eminent French observer, has noticed a behavior of those portions of the sun's surface around a spot, which seems to imply the existence of a downward current. More recently Lockyer, in this country, has made a very important observation of a similar kind. A tongue of faculous matter projecting over a spot was observed to lose its brilliancy very rapidly, so as ultimately to seem less brilliant than any portion of the penumbra. At the same time it seemed to be "giving out," as it were, at its end, and a portion of the umbra between it and the penumbra appeared to be veiled with a stratus cloud evolved out of it.

We ought likewise to mention the excellent and numerous observations of Pastorff, preserved in the library of the Royal Astronomical Society, also those of Captain Shea, both of which the Council of that Society have placed at our disposal. Professor Wolf, of Zurich, has collected data for establishing the periodicity of sun-spots before the commencement of Schwabe's observations. Also, the Rev. J. Howlett, in this country, has produced a large series of drawings of the sun's surface on a large scale, and of exquisite delicacy of delineation, which will no doubt prove of much value; and, finally, this field of research is one that has been occupied by many observers in all parts of the world, so that we may hope with some confidence for a speedy increase of our knowledge in this very important branch of physical astronomy.

12. *Composition of solar atmosphere.*—Before concluding this very brief historical sketch, we ought to allude to the discovery of Kirchhoff and Bunsen, who, by means of the spectroscope, have proved that many familiar substances, such as sodium, iron, magnesium, &c., exist in the atmosphere of our luminary in the state of vapor.

§ III. *Materials at the author's disposal.*

13. We now proceed to describe what materials we have at our disposal for the purpose of these investigations.

In the first place, Mr. Carrington has very kindly put into

our hands all his original drawings of sun-spots. These extend from November, 1853, to March, 1861; and in them the sun's disk is represented on the scale of one foot in diameter, while, for each spot, the apparent position on the disk, as well as the proportion in size to the whole surface, is accurately delineated. We hope, in our investigations (as far as spots are concerned), to make much use of these pictures by Carrington; they do not, however, afford us any information with regard to faculæ.

More recently we have received into our hands the magnificent collection of drawings of the sun made by Hofrath Schwabe, of Dessau, during the course of about forty years,—this distinguished observer having generously placed these in the possession of the Royal Astronomical Society, for the use in the meantime of the Kew Observatory.

Our materials are, moreover, derived from the pictures taken by the Kew heliograph. This instrument, with its various adjustments, has already been described by Mr. De la Rue in the Bakerian Lecture for 1862; and it is therefore unnecessary to give a further description of it here. A few pictures were taken by this instrument at the Kew Observatory in the years 1858 and 1859. In July, 1860, it was in Spain, doing service at the total eclipse. In 1861, a few pictures were taken at Kew; while, from February, 1862, to February, 1863, the instrument was in continuous operation at Mr. De la Rue's private observatory at Cranford; and from May, 1863 until the present date, it has been in continuous operation at Kew, under Mr. De la Rue's superintendence. It is right to mention that for the perfection of these pictures much credit is due to the late Mr. Welsh and to Mr. Beckley, under whose immediate supervision the pictures at Kew have been taken by a qualified assistant, Miss Beckley.

§ IV. *Method of reduction.*

14. These are the materials at our disposal; and it may here be desirable to state in a few words the principle by which we shall be guided in our reduction of these materials. In the progress of this branch of knowledge, observers have been led to recognize certain laws, which represent the average behavior of sun-spots; but to each of these laws there are individual exceptions. In this state of things it is probable that our knowledge of the subject will ultimately be advanced, not only by a study of those groups which behave in a normal manner, but also by a study of those which are exceptions in their behavior to the general rule; and on this account it has been thought desirable to publish the results in such a way that any one may be able as far as possible to study the appearance and behavior, in fact, the whole history of any one group. Setting aside, in the meantime, Schwabe's drawings for future consideration, we propose

to adopt the following plan of publication for Carrington's pictures and for those of the Kew heliograph. In discussing Carrington's observations, we shall of course adhere to the numbering of his different groups, which he has given; and as he has also exhibited a pictorial history of each of these groups in his published volume, in which the spots are represented, though on a smaller scale than in his original drawings, all that is necessary on our part is to accompany any remark we may make regarding one of Carrington's groups with the number of that group as given by him.

Next with regard to the Kew pictures. Beginning with the first picture in 1858, it is our intention to number each group of spots in the same manner as Carrington, calling the first No. 1, and so on upwards to the present date. It is also our intention ultimately to publish carefully copied representations of each of the Kew groups; but these are not yet ready. We think it well, however, to give at once the numbers of the groups, coupled with the dates at which each group was first seen, and also to make use of these numbers in our present paper in anticipation of the forthcoming pictorial representations, which, when they appear, will enable our readers to judge for themselves of the truth of our remarks.

[The table and some comparisons of the Kew observations with those of Schwabe are omitted.]

§ V. *Two classes of investigations.*

18. Our investigations may be divided into two classes:

(1.) Those in which remarks are made regarding the behavior and appearance of spots and faculæ, and generalizations deduced therefrom, which do not involve accurate measurements.

(2.) There are, however, certain results in order to obtain which it is necessary to make use of accurate measurements of the position of spots: such are those from which Carrington has deduced the proper motion of spots on the sun's surface. Probably for this class of observations no better method can be adopted than that so ably pursued by him; but since we have materials at our disposal embracing accurate photographic delineations, we are perhaps called upon to attempt corrections which he has not applied.

19. *Correction for solar atmosphere.*—The most important of these is the correction due to the refraction of the solar atmosphere, which Carrington has indicated, but without applying it, in his large volume. There are evident proofs of the existence of such an atmosphere; for

(1.) In the Kew photographs the central portion of the disk uniformly indicates a greater luminosity than the borders, as if the rays at the borders had to pass through a large extent of

atmosphere. It is worthy of remark that the temperature of this atmosphere must be lower than that of the photosphere; otherwise the absorption which it occasions would be counterbalanced by its radiation.

(2.) The beautiful discovery of Kirchhoff leads to the same conclusion, since, in order to account for the dark lines of the solar spectrum, it is necessary to suppose the existence of a solar atmosphere of a lower temperature than the source of light.

(3.) The red flames which are visible during a total eclipse, and which have been proved to belong to the sun (Art. 9), indicate the existence of a solar atmosphere extending in some instances as far as 72,000 miles above the photosphere. This is confirmed by the nature of the light which these flames emit. Mr. De la Rue has found that this light is very rich in actinic rays, so much so that he was able to photograph at least one protuberance which was not visible to the eye. Now it is precisely this description of light which characterizes the electric discharge in which gaseous matter appears in a highly heated state.

20. Let us now endeavor to show the nature of those corrections which are rendered necessary by solar refraction.

(1.) A solar atmosphere will make the sun's photosphere to appear larger than it really is; but the angular distance between two points, each near the center of the visible disk, will not be appreciably altered. This will introduce a slight error into the calculated position of any point, since in such a calculation we make use of the sun's apparent angular diameter, which is greater than his true diameter.

(2.) Apart from this, an error will be introduced into the calculation of the solar latitude and longitude of a point, this error depending upon its position in the visible disk, and being greater for those points which are at a distance from the center.

§ VI. *Questions to be answered in the present paper.*

21. In the present paper we shall attempt to answer the following questions:—

(I.) Is the umbra of a spot nearer the sun's center than its penumbra? or, in other words, is it at a lower level?

(II.) Is the photosphere of our luminary to be viewed as composed of heavy solid, or heavy liquid matter? or is it rather of the nature of a cloud? A short explanation will render evident the meaning of this question. There are two types, either of which we may conceive as representing the solar photosphere: we may, in the first place, suppose it to be a solid or liquid plane more or less uneven, with a heavy atmosphere above it. This atmosphere may be composed either of quite different materials from those of the liquid plane, or it may contain some of the materials of the plane in a state of vapor. Our own ocean is

an example of this type, the air above it being composed chiefly of materials different from those of the ocean, but containing also aqueous vapor. On the other hand, we may imagine the sun's photosphere to resemble a cloud, the characteristic of which is solid or liquid particles of a greater or less size existing in a gaseous atmosphere, composed to a greater or less extent of the materials of the cloud. These points must be determined by observation alone. We must ask if the appearances presented by the sun's photosphere lead to the conclusion that it is an uneven plane of heavy liquid or solid matter, or do they induce us to imagine that it is rather of the nature of a cloud?

(III.) Is a spot, including both umbra and penumbra, a phenomenon which takes place beneath the level of the sun's photosphere or above it?

22. In the first place, therefore, and to answer the first question, let us see what will happen if the umbra of a spot be nearer the sun's center than the penumbra?

If the umbra be lower than the penumbra, when a spot passes over the sun's disk, the umbra will always appear to encroach upon that side of the penumbra which is directed toward the visual center of the sun's disk; and this effect will be lessened through the refraction caused by a solar atmosphere, but we cannot conceive that it will be wholly obliterated. If therefore the umbra is appreciably at a lower level than the penumbra, we are entitled to look for an apparent encroachment of the former upon the latter on that side which is nearest the visual center of the disk. This in fact was the phenomenon which Wilson observed, and which led him to the belief that the umbra was nearer the sun's center than the penumbra.

23. In the following six sub-tables the effect of foreshortening is estimated in the direction from left to right, this being the direction in which spots advance across the visible disk by rotation; and for this purpose the whole surface of the sun has been divided into six portions, comprising 30° each.

[These sub-tables are omitted, the general results from them being given in the following summaries.]

Result of TABLE II_a. *Showing the effect of foreshortening in the direction left and right of the central line.*

a. Giving the mean ratios between the two sides of the penumbra.

Left of central line.					
Within 30° from left limb.		Between 30° and 60° from left limb.		Within 30° from central line.	
No. of spots observed.	The penumbra on the *right* side of a spot being equal to unity, that on the *left* is equal to	No. of spots observed.	The penumbra on the *right* side of a spot being equal to unity, that on the *left* is equal to	No. of spots observed.	The penumbra on the *right* side of a spot being equal to unity, that on the *left* is equal to
130	1·8	119	1·4	88	1·2

Result of TABLE II$_a$.—continued.

Right of central line.					
Within 30° from central line.		Between 30° and 60° from right limb.		Within 30° from right limb.	
No. of spots observed.	The penumbra on the *left* side of a spot being equal to unity, that on the *right* is equal to	No. of spots observed.	The penumbra on the *left* side of a spot being equal to unity, that on the *right* is equal to	No. of spots observed.	The penumbra on the *left* side of a spot being equal to unity, that on the *right* is equal to
91	1·2	77	1·3	100	1·6

b. Giving the percentage of cases, out of 530 observations in all, which are in conformity with the assumption, that the umbra is nearer to the center of the sun than the penumbra, and of cases which are against it.

Left of central line.											
Within 30° from left limb.				Between 30° and 60° from left limb.				Within 30° from central line.			
For.		Against.		For.		Against.		For.		Against.	
No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
119	92·3	10	7·7	99	90·0	11	10·0	55	86·1	9	13·9
Right of central line.											
Within 30° from central line.				Between 30° and 60° from right limb.				Within 30° from right limb.			
For.		Against.		For.		Against.		For.		Against.	
No.	Per cent	No.	Per cent	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Percent.
46	74·2	16	25·8	55	79·7	14	20·3	82	85·4	14	14·6

The whole number of cases observed is 605; excluding herefrom 75, where the penumbra is equal on both sides, there remain 530, of which 456, or 86·04 per cent are *for*, and 74, or 13·96 per cent are *against* the assumption.

Result of TABLE II$_b$.

Showing the effect of foreshortening in the direction above and below the Equator.

Above the equator.					Below the equator.				
The lower part of the penumbra of each spot being equal to unity, the upper part is equal to	For assumption.		Against assumption.		The upper part of the penumbra of each spot being equal to unity, the lower part is equal to	For assumption.		Against assumption.	
	No. of cases.	Per cent.	No. of cases.	Per cent.		No. of cases.	Per cent.	No. of cases.	Per cent.
1·54	42	87·5	6	12·5	1·33	30	73·2	11	26·8

The whole number of cases considered is 89, of which 72, or 80·9 per cent, are for, and 17, or 19·1 per cent, against the assumption, that the umbra is nearer to the center of the sun than the penumbra.

In table II$_b$, since the relative disposition of umbra and penumbra is estimated in directions parallel to circles of solar longitude, only spots having a higher solar latitude have been considered.

It will be seen that the results of tables II$_a$ and II$_b$ are decidedly in favor of Wilson's hypothesis.

24. Let us now endeavor to answer the second question—"Is the photosphere of our luminary to be viewed as composed of heavy liquid or solid matter, or is it of the nature of a cloud?" One characteristic of the sun's surface is the appearance (especially in connexion with spots) of faculæ, or patches of a brightness greater than that of the photosphere immediately around them, this difference in brightness being much more conspicuous near the limb than near the center. One explanation of these phenomena would be, that the luminous matter of the sun has been thrown up to a great elevation in order to form faculæ. This would account for the greater comparative luminosity of faculæ near the border. For we have already mentioned (Art. 19) that the absorbing effect of the solar atmosphere is very perceptible near the border, where the light reaching us has to travel through a greater thickness of atmosphere; and hence, if the luminous matter be thrown up to a great elevation, it will, near the border, escape a great portion of this atmosphere, and will therefore appear relatively much brighter than the surface around it. On the other hand, very little will be gained when the matter is thrown up near the visual center, where we may imagine the atmospheric absorption to be comparatively small. The idea that faculæ are portions of the photosphere raised above the general surface, appears to be confirmed by stereoscopic pictures of spots obtained by Mr. De la Rue, where the faculæ appear as elevated ridges surrounding the spots. Accepting this conclusion, we next remark that faculæ often retain the same appearance for several days together, as if their matter were capable of remaining suspended for some time.

Now if we suppose that such faculæ represent the ordinary luminous matter of the sun, the facts above recorded would appear to throw much light upon the nature of the solar surface, since we cannot imagine faculæ to be the most elevated positions of a liquid ocean, which has been pushed high up into the solar atmosphere, or to be portions of matter projected from such an ocean. Such an hypothesis would appear to be inconsistent with the fact that the faculæ retain their appearance unchanged for days together. At any rate we venture to think that such an hypothesis would not readily be received, and that, according to the rules which ought to guide our judgment in a case like the present, it ought to be set aside if we can find a more plausible explanation. Such an explanation would appear to consist in supposing that faculæ, and, indeed, the whole photosphere of our luminary, are more of the nature of a cloud. A cloud has been defined by Sir J. Herschel to consist of solid or liquid matter, formed from the condensation of a vapor not *floating* in æquilibrio, but sinking in a gaseous medium of less specific gravity than itself—sinking, however, with extreme slowness,

owing to the minuteness of its particles, and consequent (relatively) enormous resistance of the air. This illustrious savant is disposed to think that the consideration of Cagniard de Latour's experiments on the vaporization of liquids under high pressure would incline us to regard the solar faculæ as large aggregations of *bonâ fide solid matter* of a high degree of fixity, and in masses like gigantic soot-flakes of any form and magnitude, which, when formed, settle down to such a level as corresponds to their density when they rest *in æquilibrio* in a gaseous fluid of their own specific gravity. We do not wish either to accept or to reject this hypothesis, but would frame the following statement, which also includes this view of the case: *Solar faculæ consist of solid or liquid bodies of a greater or less magnitude, either slowly sinking or suspended in æquilibrio in a gaseous medium.*

25. In connection with this part of our subject it will be well to investigate the relative position of spots and their accompanying faculæ; and this is done in the following table for all the Kew pictures available for this purpose.

[Table III is omitted.]

Result of TABLE III.

Facula entirely or mostly *left* of spot.		Facula entirely or mostly *right* of spot.		Facula *all round* or between the spots.	
No. of cases.	Per cent of the whole.	No. of cases.	Per cent of the whole.	No. of cases.	Per cent of the whole.
584	51·4	45	4·0	508	44·6

26. It appears from the result of table III, that out of 1137 cases 584 have their faculæ either entirely or mostly on the left, while 508 have it nearly equally on both sides, and only 45 mostly on the right. Hence we see that faculæ are on an average to the left of their accompanying spots. The most obvious explanation of this would be that the faculæ of a spot have been uplifted from the very area occupied by that spot, and have fallen behind to the left from being thrown up into a region of greater velocity of rotation. All this is quite in accordance with our hypothesis regarding the nature of faculæ. We would likewise here remind our readers that we know from the observations of Kirchhoff that the sun's atmosphere contains vapors of substances, such as iron, which are condensed into the liquid or solid state at a comparatively high temperature. Now is it not natural to suppose that in the sun's photosphere we do really see such vapors so condensed, and very unnatural to imagine that such vapors are seldom or never condensed, and that what we really see is an incandescent plain underlying these vapors?

27. Let us now attempt to answer the third question: Is a

spot including both umbra and penumbra a phenomenon which takes place beneath the level of the sun's photosphere or above it? To decide this question, let us state that there are a good many instances in which a spot breaks up in the following manner. A bridge of luminous matter of the same apparent luminosity as the surrounding photosphere, and unaccompanied by any penumbra, appears to cross over the umbra or center of a spot. There is good reason to think that this bridge is really above the spot; for were the umbra an opaque cloud, and the penumbra a semiopaque cloud, both being above the sun's photosphere, it is unlikely that the spot would break up in such a manner that the terrestrial observer should not perceive some penumbra accompanying the luminosity. Again, detached portions of luminous matter appear to move across a spot without producing any permanent alteration. We are on these accounts disposed to think that a spot including both umbra and penumbra is a phenomenon which takes place beneath the level of the brighter part of the sun's photosphere.

28. Let us here recapitulate the answers we have given to our three questions.

(1.) The umbra of a spot is nearer the sun's center than its penumbra, or, in other words, it is at a lower level.

(2.) Solar faculæ, and probably also the whole photosphere, consist of solid or liquid bodies of greater or less magnitude, either slowly sinking or suspended *in æquilibrio* in a gaseous medium.

(3.) A spot including both umbra and penumbra is a phenomenon which takes place beneath the level of the sun's photosphere.

§ VII. *Concluding remarks.*

29. It would thus appear that the central part of a spot is nearer the sun's center than the penumbra, and that both the umbra and penumbra are probably beneath the general level of the surrounding photosphere. Now the umbra or lowest part of a spot is much less luminous than the general photosphere. But what does this probably imply, according to the laws with which we are acquainted? It implies that in a spot there is probably some matter of a lower temperature than the photosphere. For is it not now recognized as a law, that if a substance, or combination of substances, of indefinite thickness and surface of small reflecting power have all its particles at a certain fixed temperature, this substance will give out nearly all the rays of heat belonging to that temperature? Now the sun, even when we look into a spot, is certainly a substance of indefinite thickness; and since a spot appears much less luminous than the ordinary surface, ought we not to conclude either that we there view matter of a lower temperature than the ordinary

surface, or that the matter which appears within a spot has a very high reflecting power compared to the ordinary matter of the photosphere? This last supposition is an unlikely one, and the probability is that in a spot we view matter of a lower temperature than the photosphere.

30. Presuming this to be the case, it appears to imply one of three things.

(1.) Either the general body of the sun at the level of the bottom of a spot is of a lower temperature than the photosphere;

(2.) Or the lower temperature is produced by some chemical or molecular process which takes place when a spot is formed;

(3.) Or it is produced by matter coming from a colder region.

The first of these suppositions will not be generally received unless we are fairly driven to accept it.

The second hypothesis has already been started to account for the lower temperature of a spot; but we think that, according to the laws by which we should be guided in receiving or rejecting an explanation in a case of this nature, this idea ought to be rejected.

No doubt, if we knew of a case of the production of low temperature, and had at the same time an independent proof of some chemical or molecular process, such as evaporation, it would be quite allowable for us to associate the chemical or molecular process with the production of cold as at any rate the most likely hypothesis; but we do not advance in our explanation of the low temperature by attributing it to an imaginary process of the existence of which we have no proof, and which is equally mysterious with the phenomenon for which it is supposed to account. Rather let us see if this reduction of temperature can be explained by any other phenomenon of the existence of which we have independent evidence. This leads us to consider the third hypothesis, which supposes that the reduction is produced by matter coming from a colder region. Now, in the first place, we have such a region in the atmosphere above the photosphere, which (Art. 19) we have shown to be of a lower temperature than the photosphere itself. Again, the observations of Chacornac and Lockyer on the behavior of the matter surrounding a spot appear to suggest the existence of a downward current, which is therefore a current from the colder regions above.* On the other hand, the proper motion of spots observed by Carrington is in favor of this hypothesis, since a current coming from a region of greater to a region of less absolute velocity of rotation would be carried on forward, and most so nearest the equator; and this is precisely the motion of

* Does not the observation by Lockyer of the facula "giving out" appear also to indicate that the lower regions of a spot are in reality hotter than the surface, leaving the inferior luminosity to be accounted for by the downrush of a cold atmosphere from above?

spots observed by Carrington. Again, we have seen (Art. 26) that the faculæ fall behind; so that we may imagine two currents to be engaged in the formation of a spot,—the one an ascending current carrying the hot matter behind, the other a descending current carrying the cold matter forward. One advantage of this explanation is that all the gradations of darkness, from the faculæ to the central umbra, are thus supposed to be due to the same cause—namely, the presence to a greater or less extent of a comparatively cold absorbing atmosphere.

31. It is but just to ourselves and to M. Faye, to mention that both have imagined the phenomenon of sun-spots to be due to ascending and descending currents. M. Faye's hypothesis was published a little before ours; but we shall readily be believed when we state that an idea of this kind presided over the construction of table III, in which we have proved that the faculæ are, on an average, to the left of their accompanying spots. It was not, however, until a short time before the publication of the abstract of this paper by the Royal Society, that, by discussing the subject together, we had matured our views so far as to connect the descending current, not only with Carrington's proper motion, but also with the presumed lower temperature of a spot. In this last respect our hypothesis differs entirely from that of M. Faye, who does not imagine that the inferior luminosity of a spot indicates the presence of matter at a lower temperature than the photosphere.

32. In conclusion, we would venture to suggest that if the photosphere of the sun be the plane of condensation of gaseous matter, this plane may be found to be subject to periodical elevations and depressions in the solar atmosphere. It may be that at the epoch of minimum spot-frequency this plane is uplifted very high in the solar atmosphere, so that there is comparatively little cold absorbing atmosphere above it, and therefore great difficulty in forming a spot. If this were the case we might expect a *less* atmospheric effect or gradation of luminosity from the center to the circumference at the epoch of minimum than that of maximum spot-frequency. Perhaps on some future occasion we may be able to produce evidence of this, and even of the unequal atmospheric effect of the two limbs of the sun at the same time; but in the meantime we shall content ourselves with suggesting this to the observers of our luminary as a simple inquiry that may possibly prove productive.

33. We are especially desirous of bringing under the early notice of the scientific world the accumulation of observations we are making, in order that others may put forth their own conjectures in elucidation of solar physics. In venturing the opinions we have stated, we do so with some reserve, and with the conviction that possibly they may hereafter require modifications.

ART. XX.—*On the Subterranean Sources of the Waters of the Great Lakes;* by GEORGE A. SHUFELDT.

IF we take down the map of North America, and follow around the borders of our chain of Great Lakes, we find that the tributaries for supplying the mighty torrent of water which pours in immense volumes over the Falls at Niagara, and thence through the St. Lawrence to the sea, are few in number and insignificant in effect. Lake Superior, the largest body of fresh water in the world, has an area of 32,000 square miles and a mean depth of one thousand feet. There are a few small streams, none worthy of the name of rivers, which find their outlet in this lake—the St. Louis and Ontonagon are the largest of these; but there is probably not water enough discharged into the lake to make up for the atmospheric absorption and evaporation. The entire State of Wisconsin, even from the very borders of Lake Superior, is drained by rivers, which flow into, and are tributaries of the Mississippi. These are, in chief, the Wisconsin River, the Black, Chippewa, Fox and Rock Rivers, the waters of which all flow southward, to the Gulf of Mexico. The whole State of Minnesota with its thousands of lakes and streams may be called the mother of the Father of Waters—for all of her waters which do not gather into the great Red River of the North are discharged into the Mississippi, and do not contribute to keep up the supply of Lake Superior; and on the northern shore of the lake, in the British possessions, there are no rivers which flow in this direction. Here the current is the other way and the streame find their way to Hudson's Bay and other more northerly seas. The outlet of Lake Superior is the River St. Mary's—a stream of considerable magnitude—which discharges the surplus waters of the Lake in the direction of Lake Huron. Lake Superior is 627 feet above the sea level.

If we examine the surroundings of Lake Michigan we shall find the evidences of this theory still more striking. This lake has an area of 22,400 square miles, and a mean depth of 900 feet. It is above the sea level 578 feet or forty-nine feet below Lake Superior. It is also an immense body of water, whose sole apparent sources of supply are found in a few small streams which flow into it from the State of Michigan.

The largest of these are the Grand and Manistee rivers; from Wisconsin there is only one small stream, the Milwaukee river at Milwaukee. From Illinois there is only the Chicago river, a sluggish stream without a current; and indeed there is, at only ten miles distance from the banks of the lake south and west,

the water-shed called the Summit, which separates the waters which flow into the St. Lawrence from those which flow into the Gulf of Mexico, and from the southern slope of this Summit, flowing southward, is the Aux Plaines river, a tributary of the Illinois. So that Lake Michigan gets no water from Illinois, but a trifle from Wisconsin, and very little from Michigan. And yet the straits of Mackinaw carry off a large quantity of water from this lake, and Lake Michigan furnishes its due proportion of the great current which passes over the Falls of Niagara. Now the question arises, whence comes this great volume and mass of running water?

Geologists are tolerably familiar with the subject of underground streams and water courses. They know that the crust of the earth is full of these streams, and although from the fact that they are generally concealed from sight, there must be considerable speculation concerning them, yet there are cases, such as in the Mammoth Cave, Kentucky, the Adelsberg mountains in Switzerland, and numerous artesian wells scattered all over the world, the lost rivers on our western prairies, &c., from which a positive knowledge may be derived concerning the nature and history of these rock-bound rivers.

The artesian wells in London furnish now about 12,000,000 of gallons of water daily. This is the seepage of the valley in which the great city is located. The water from the whole country surrounding finds its way along the tilts and inclinations of the broken strata, below the chalk beds, in among the sands and gravel, whence it is taken by boring into the ground to the depth of about 600 feet. It does not appear probable that there are any considerable streams in this vicinity, for the entire of the underlying gravel beds seem, as it were, saturated with water, which is reached at any point of perforation.

These remarks apply to the wells of Grenelle and of Passy, in the basin of Paris, with the exception in the case of the latter that they struck an amazing stream of water eighteen hundred feet below the surface which discharges nearly six millions of gallons per day, rushing to the surface with great power and velocity. This is strong evidence, certainly, of a great underground stream at this point. The great wells of Kissingen in Bavaria, at Munden in Hanover, at Louisville in Kentucky, Charleston, S. C., and hundreds of others, many of which are two thousand feet deep, discharging great volumes of water—all tend to demonstrate the fact that the crust of the earth is penetrated in all directions and at all depths with these streams and water courses.

Adopting this as a conceded fact, let us once more turn to the map of North America and note particularly the point where the thirty-second degree of west longitude crosses the forty-fourth parallel of north latitude. Within a radius of five hundred

miles, of which this is the center, will be found the great water producing region of the West. In this elevated and comparatively uneven surface of the country, nearly all of the great rivers of the West have their sources and fountain-heads. First the Missouri, with its innumerable branches and tributaries, among which are the Yellowstone and the North Fork of the Platte, the Arkansas, the Red River, the Rio Grande, all flowing from the eastern and southern slopes of the Rocky Mountains and finding their way through thousands of miles of country to the Gulf of Mexico. On the western slope is the Rio Colorado, which empties into the Gulf of California, and which is formed by the union of the Grand and Green rivers, the sources of which are also within the territory above mentioned. The same statement is true of the Columbia river flowing through the State of Oregon into the Pacific, and of the other great streams and rivers which flow northward and westward into the Pacific and the Northern oceans. Thus the knowledge we already possess of the surface streams of this great extent of territory all tends to demonstrate the truth of the theory in relation to the water producing region, its location, extent and capacity, and also that on the surface there is but comparatively a small amount of this water which finds its way into our Great Lakes.

It is a well-known fact to travellers on our western plains, that large streams, often rivers in size, suddenly disappear, falling away into great fissures and chasms, sometimes reappearing, but more frequently lost forever; where and in what manner does this water find an outlet? What becomes of the mass of water which falls upon the earth and is absorbed by the soil and the rocks below the beds of rivers and streams? The crust of the earth abounds in water to unknown depths, and from the nature of the element, it must create for itself ways and courses of travel, as plainly beneath as upon the surface. And now, if the Great Lakes are not supplied by means which are upon the the surface and apparent to the eye, it follows as a natural consequence that their sources of supply must be underneath the ground. The outlet of these lakes discharges an enormous quantity of water, the visible inlets are mere trifles in comparison—and thus there seems to be no other conclusion on the subject but that the water supply comes from below the surface of the ground. This water probably finds inlets at different points on the bottoms of the lakes, and maintains the supply with as much certainty and regularity as if the streams were running on the surface of the ground. This theory is further, and I think more particularly demonstrated by the great mass and volume of water which is now being discharged by the Chicago artesian wells. These are over seven hundred feet deep—nearly penetrating the earth to a line parallel with the bottom of Lake Michigan—are located in

no great valley or depression, such as the basins of Paris or London. The water has a head of nearly one hundred and twenty-five feet above the level of the lake; is much colder than the mean temperature of the location of the wells, being now 57 degrees Fahrenheit; these facts tending to show that it must come from a more elevated region of country, and also from a higher latitude. There are two other facts corroborative of this point. When the water was first struck the temperature was 59 degrees Fahrenheit; it has fallen now two degrees, or to 57. Then, the first analysis of the water exhibited 72 grains to the wine gallon of mineral matter held in solution; the second analysis, made only one year afterwards, showed only 56 grains of the same matter. These facts, taken in connection with the great head of the water, seem to establish exclusively that it comes from some remote region of the north or northwest.

It is also probable that the great under-ground stream, penetrated by these wells, once discharged its waters into the bottom of Lake Michigan; but this outlet was closed by the upheaval of the earth's crust, which is visible at the point of the location of these wells, and at the present time there is no outlet except the artificial one made by the drill. This supposition is proved by the head and the great force and power of the water, for if it had a lower outlet, anything like the size of the stream, it would not show a head much, if any, above the surface of the ground, and it is also sustained by the facts mentioned above—the decrease of the temperature of the water from fifty-nine degrees to fifty-seven degrees, and the diminution in quantity of mineral matter held in solution—the latter fact seeming clearly to prove that prior to the time when the drills penetrated the stream, the water had dissolved and absorbed a large quantity of the soluble matter of the rocks with which it came in contact in its state of rest. As soon as an opening or outlet was made, and a quantity of water was discharged, this mineral matter decreased in proportion, and the probability now is that the water will become softer and purer as the amount discharged becomes greater, and that eventually, and probably at no distant day, the water will come from its fountain-head, simply filtered and purified by its passage through the sandstone and gravel beds.

That the outlet of this stream into Lake Michigan was closed by the upheaval of the earth's crust, is a probable conclusion, which can be verified by an inspection of the grounds on which these wells are located. The surface here is only some seven or eight feet above the level of the surrounding prairie; but geologically or stratigraphically, it is nearly one hundred and fifty feet above the common level of Chicago, that is, at about one mile distant eastward and into the city. We bore

into the soil nearly one hundred and fifty feet before reaching the same rock, which is here exposed upon the surface, and at the well bored at the Chicago Distillery Company's premises on the North Branch, they penetrated the Joliet marble at a depth, I believe, of one hundred and eighty-six feet, which, at the other point is only twenty-nine feet from the surface; this and various other facts show the nature and extent of this convulsion, and that it was no difficult feat of nature to dam up this comparatively trifling underground stream, and leave its waters pent up in the rocks and caverns for the future use and benefit of man.

I do not know that these speculations will be of sufficient interest to be made public, but they may have the effect of directing some abler pen to the solution of the problem as to the sources whence the Great Lakes derive their supply of water.

Chicago, November, 1866.

ART. XXI.—*Experiments on the Influence of various Organic and Inorganic Bodies upon Germination and Vegetation;* by M. CAREY LEA, Philadelphia.

THE following experiments were made to determine how far the germination of seeds, and subsequent vegetation of the plants, would be controlled or influenced by the action of acid, alkaline and neutral bodies in solution in the water with which the seeds were moistened. It will be seen that the action of the strongest of our acids is insufficient to prevent germination when sufficiently dilute. And that the same may be said with respect to some of our most powerful oxydizing and reducing agents.

The experiments were made by tying pieces of very thin muslin over glass vessels filled so full that the muslin dipped into the liquid. Grains of wheat were placed on this muslin, an equal number (20 perfect grains) on each. The capacity of the glasses was in every case 12½ ounces, and the water was replaced as fast as it evaporated. There was added respectively to each as follows:

No. 1. 1 drop sulphuric acid.
2. 2 drops nitric acid.
3. 3 " hydrochloric acid.
4. 5 grs. bicarbonate of potash.
5. 5 " dry carbonate of soda.
6. 10 drops of rather weak liquid ammonia.
7. 5 grs. bromid of ammonium.
8. A pair of zinc and copper plates connected above the surface by a wire, and plunged in plain water.

No. 9. Same, acidulated 3 drops hydrochloric acid.
10. Plain water for comparison.
11. 5 grains sulphite of soda.
12. " " chlorate of potassa.

The results were as follows. At the end of

48 *hours*—Germination evident in all. Most advanced, 4, 10, 11; medium, 2; least, 1, 3, 5, 6, 7, 8, 9, 12.

3 *days*—Most advanced, 4, 10, 11; medium, 5, 7, 8, 9, 12; least, 1, 2, 3, 6.

4 *days*—Most advanced, 4, 10, 11; medium, 5, 7, 12; less, 2, 6, 8, 9; least, 1, 3.

5 *days*—Most advanced, 4, 10, 11; medium, 5, 6, 7, 8, 12; less, 1, 9; least, 2, 3.

6 *days*—Most advanced, 4, 10, 11; a little less, 5, 6, 7, 8; much behind, 1, 2, 3, 9.

Some curious deductions are to be drawn from these results. *Nitric acid* did not at first very strongly affect the growth, less than seven other substances, then eventually its influence became much more felt.

Bicarbonate of potash was the least injurious of all the substances tried, next came *sulphite of soda*, and next *carbonate of soda*.

No saline or other substance included acted in any way as a stimulant, the product of the plain water as an average was fully up to any of the rest, though as will hereafter be seen, the largest plant was formed in another vessel.

In the observations just made I have endeavored to show the daily course of action; in the following table I have summed up the total effects at the end of seven days, when experiment was discontinued.

Total results at the end of seven days.

	Proportion of substance in 100 of water.	No. of seeds that germinated out of 20.	Average height of young plants.	Total amount of vegetation.	Proportional amount of vegetation, taking that in plain water as 100.
1.	SO_3, 0·016,	18	·3	5·4	16·8
2.	NO_5, 0·033,	14	·2	2·8	8·7
3.	HCl, 0·050,	3	·3	·9	2·8
4.	$KO\,2CO_2$, 0·083,	16	2·	32·	100·
5.	$NaO\,CO_2$, 0·083,	14	1·5	21·	65·6
6.	Ammonia,	13	1·5	19·5	60·9
7.	NH_4Br, 0·083,	11	1·4	15·4	48·1
8.	Zn and Cu plates,	16	1·3	20·8	65·0
9.	Same with 3 drops HCl.	8	·4	3·2	10·
10.	Plain water,	16	2·	32·	100·
11.	$NaO\,SO_2$, 0·083,	12	2·	24·	75·
12.	$KO\,ClO_5$, 0·083,	13	·5	6·5	20·3

This table shows:

That an oxydizing agent, chlorate of potash (12), is not more injurious than a reducing one, sulphite of soda (11), to germination, but after germination it kept down vegetation to one-fourth.

That free acids are much more injurious than alkalies, especially hydrochloric acid (3).

That the presence of an electric pair did not check germination, but reduced vegetation by one-third.

That the presence of free sulphuric acid had no injurious influence upon germination, actually a larger proportion of seeds started than with pure water, whereas with hydrochloric acid only three seeds germinated out of twenty. But sulphuric acid reduced vegetation to one-sixth, hydrochloric to 2·8 per cent.

With bicarbonate of potash, precisely the same number germinated as with plain water, and attained precisely the same height.

In (9) the HCl acted less energetically than in (3), doubtless because it was rapidly taken up by the zinc.

Plants in the sulphite of soda attained the same height as those in plain water. But the number germinating was one-fourth less.

A second set of trials was made, in which a number of other substances were experimented upon, and at the same time sulphuric acid was added in much smaller quantity, and sulphite of soda in much larger. Capacity of the vessel as before, 12½ oz.

No. 1, Plain water.
2, Cane sugar, 30 grains.
3, Gum, 30 grains.
4, Glycerine, 1 fluid drachm.
5, Sulphuric acid, ¼ drop.
No. 6, Citric acid, 5 grains.
7, Sulphite of soda, 20 grains.
8, Permanganate of potash, 2 grains.
9, Nitrate of ammonia; 20 grs.

The object of this series was to include in the experiments certain organic substances such as the three first on the list, a vegetable acid, and some salts whose influence might be active and characteristic.

At the end of thirteen days, during which the weather was very cold (Dec. 10 to Dec. 23), the following was the condition of affairs.

Nos. 2 and 4 (cane sugar and glycerine) were as far advanced as the plain water (No. 1), but no further. These substances therefore had not stimulated either germination or early vegetation in the wheat seeds.

In 3 (gum solution) fewer seeds germinated than in either of the foregoing, but the most advanced plants were *fully one-half higher* than any in 1, 2, or 4.

Nos. 7 and 9 (sulphite of soda and nitrate of ammonia) were somewhat in advance of those in plain water, but not very much.

In 6 (citric acid) a large number germinated, and appeared

healthy, but they did not obtain one-fourth the height of those in No. 1, and what was very remarkable, they *formed no roots at all.*

In 5 (sulphuric acid) the plants were more advanced than in the citric acid, and had healthy roots extending down into the liquid.

In 8 (permanganate of potash) the condition of affairs most resembled that in the citric acid. In both the seeds had germinated and produced healthy looking plants an inch in height. But *no roots whatever* had been formed in either case.

Some of the above sets of seeds were allowed to vegetate for a month, and developed curious results.

Those plants which grew in the vessels containing solutions of cane sugar, gum, and glycerine respectively, grew as fast and flourished as well as those in plain water, but it could scarcely be said that at the end of the month they presented any superiority.

But whilst the roots of the plants in plain water, in gum, and in glycerine, reached to the very bottom of the vessel, becoming four to five inches long, those in the cane sugar did not exceed an inch in length, just dropping below the surface of the water, which had become lowered by spontaneous evaporation, and this although the plants were as high as in the others just mentioned, viz., six to eight inches, and as numerous and healthy in every respect. This would seem to indicate that they received their nutriment in a more concentrated form, if it were not that these plants, though equally large and healthy as those in plain water, exhibited no superiority over them.

ART. XXII.—*Contributions from the Sheffield Laboratory of Yale College.*—XIII. *On Native Crystallized Terpin;* by S. W. JOHNSON.

IN October, 1866, the writer received from Wm. M. Gabb, Esq., of the Geological Survey of California, a small quantity of crystals found in "cavities near the core of a semi-decomposed pine stump that was buried three or four feet below the surface in Shasta Co., California." The crystals were discovered by Mr. Voy of San Francisco.

At the request of Mr. Gabb I have examined these crystals, which, in the sample received, were still partly adhering to a fragment of pine, where they were associated with another crystalline substance of a yellowish color and resinous aspect.

The crystals were colorless and transparent, the largest individual was three-eighths of an inch long, one-eighth of an inch wide and one-sixteenth of an inch thick. They were of brilliant luster and well terminated at the free ends. From their occurring in buried pine wood and from their general appearance, it

was at once suspected they might be identical with crystallized terpin. Their faint resinous taste and odor, not to be distinguished from that of the artificial substance, confirmed this view.

To obtain full information regarding the crystallometrical characters of the substance, I applied to my friend, Mr. John M. Blake of New Haven, to make a comparison between the native crystals and those of artificial preparation from the chemical cabinet of the Sheffield Scientific School. Some of the highly interesting results of these investigations are communicated by Mr. Blake in the paper that follows, and leave no doubt of the identity of the two substances, although their crystals are not developed in the same manner, and exhibit other physical differences which, as he states, disappear when both are recrystallized from the same solvent.*

After Mr. Blake had finished his examinations, a combustion was made on nearly the whole available substance. The hydrogen determination was lost by the fracture of the CaCl tube, but the estimation of carbon fully confirmed the conclusions previously arrived at. The combustion was effected in a tube partly filled with oxyd of copper and in a stream of oxygen, the substance itself being placed in a tray of platinum. On application of heat it swelled and afterwards vaporized completely, without blackening and without leaving a weighable residue. On the cold parts of the tube silky crystals of anhydrous terpin condensed. This deportment is characteristic of terpin.

The amount of substance burned was but 0·0975 grm. The increase in weight of the potash bulbs and tube was 0·225 grm. This gives carbon 62·93 *per cent.* The calculated quantity is 63·16 *per cent.*

The substance is therefore hydrated terpin or crystallized turpentine camphor $C_{20}H_{20}O_4+2aq$. Perhaps we should say it is one of the terpins, since, according to Berthelot, the different oils of turpentine, on hydration, yield crystals of different degrees of solubility.

The formation of this substance in the buried tree presents no difficulties, since we know on the authority of Dumas, Deville and others, that oil of turpentine in contact with water, combines with the latter in absence of acids or other powerful agents of chemical change.

Prof. Brewer, who is familiar with the timber of California, is of the opinion that the wood to which the crystals were attached is that of a pitch pine, *Pinus ponderosa.*

This appears to be the first recorded instance of the occurrence of crystallized terpin, native.

November, 1866.

* Mr. Blake has measured and figured both the native and artificial crystals and has in reserve some other valuable observations which it is to be hoped he will shortly publish.—S. W. J.

ART. XXIII.—*On the crystallization of natural Hydrated Terpin from California;* by JOHN M. BLAKE.

SOME crystals, from a buried pitch-pine log, were handed me for examination by Prof. S. W. Johnson, of the Sheffield Scientific School.

A comparison of these crystals with terpin of artificial preparation leaves no doubt that the natural substance is hydrated turpentine camphor. The natural and artificial crystals agree closely in their angles, and have the same cleavage. The position and separation of the optical axes is alike in both, and experiment shows that the two substances are supercrystallizable.

Certain observations made at first, suggested that the two specimens might not be absolutely identical, but rather isomeric hydrates, such as were supposed by Berthelot to result from isomeric oils, derived from the same or different trees. Thus, hemihedrism constantly occurred on the natural crystals, which has not been observed on the artificial. The proportional development of the planes was strikingly different. The two specimens manifested opposite pyro-electric characters, in so far that the *free-growing* extremities of the natural crystals were antilogue poles, (developed negative electricity on heating,) while those of the artificial crystals, first examined, were the reverse, or analogue poles.

On further investigation, these points of difference disappeared. By recrystallizing from alcohol and other solvents, much variation was produced in the planes. The peculiar development of the natural crystals was not indeed reproduced on the artificial, but the attachment of the latter to the support by the analogue pole, as with the natural crystals, was obtained. On recrystallizing from alcohol, natural terpin lost its hemihedral character, and in case of crystals grown radiating from a support, presented the analogue pole to the solution, like the artificial substance when deposited from the same solvent. Crystals of each, when free-growing in alcoholic solution, had the same development of the planes, and with each there was the same perceptible difference in the proportions of the planes at the two ends of a crystal, by which the poles could be distinguished; but no corresponding difference could be detected in the angles of these terminal planes.

Art. XXIV.—*On the Objects and Method of Mineralogy;* by T. Sterry Hunt, F.R.S.

(Read before the American Academy of Sciences, Jan. 8, 1867).

Mineralogy, as popularly understood, holds an anomalous position among the natural sciences, and is by many regarded as having no claims to be regarded as a distinct science, but as constituting a branch of chemistry. This secondary place is disputed by some mineralogists, who have endeavored to base a natural-history classification upon such characters as the crystalline form, hardness, and specific gravity of minerals. In systems of this kind, however, like those of Mohs and his followers, only such species as occur ready formed in nature are comprehended, and the great number of artificial species, often closely related to native minerals, are excluded. It may moreover be said in objection to these naturalists, that, in its wider sense, the chemical history of bodies takes into consideration all those characters upon which the so-called natural systems of classification are based. In order to understand clearly the question before us, we must first consider what are the real objects, and what the provinces, respectively, of mineralogy, and of chemistry.

Of the three great divisions, or kingdoms of nature, the classification of the vegetable gives rise to systematic botany, that of the animal to zoology, and that of the mineral to mineralogy, which has for its subject the natural history of all the forms of unorganized matter. The relations of these to gravity, cohesion, light, electricity, and magnetism, belong to the domain of physics; while chemistry treats of their relations to each other, and of their transformations under the influences of heat, light, and electricity. Chemistry is thus to mineralogy what biology is to organography; and the abstract sciences, physics and chemistry, must precede, and form the basis of the concrete science, mineralogy. Many species are chiefly distinguished by their chemical activities, and hence chemical characters must be greatly depended upon in mineralogical classification.

Chemical change implies disorganization, and all so-called chemical species are inorganic, that is to say unorganized, and hence really belong to the mineral kingdom. In this extended sense, mineralogy takes in not only the few metals, oxyds, sulphids, silicates, and other salts, which are found in nature, but also all those which are the products of the chemist's skill. It embraces not only the few native resins and hydrocarbons, but all the bodies of the carbon series made known by the researches of modern chemistry.

The primary object of a natural classification, it must be re-

membered, is not like that of an artificial system, to serve the purpose of determining species, or the convenience of the student, but so to arrange bodies in genera, orders, and species as to satisfy most thoroughly natural affinities. Such a classification in mineralogy will be based upon a consideration of all the physical and chemical relations of bodies, and will enable us to see that the various properties of a species are not so many arbitrary signs, but the necessary results of its constitution. It will give for the mineral kingdom what the labors of great naturalists have already nearly attained for the vegetable and animal kingdoms.

Oken saw the necessity of thus enlarging the bounds of mineralogy, and in his Physiophilosophy, attempted a mineralogical classification; but it is based on fanciful and false analogies, with but little reference either to physical or chemical characters, and in the present state of our knowledge is valueless, except as an effort in the right direction, and an attempt to give to mineralogy a natural system. With similar views as to the scope of the science, and with far higher and juster conceptions of its method, Stallo, in his Philosophy of Nature, has touched the questions before us, and has attempted to show the significance of the relations of the metals to cohesion, gravity, light, and electricity, but has gone no farther.

In approaching this great problem of classification, we have to examine—first, the physical condition and relations of each species, considered with relation to gravity, cohesion, light, electricity, and magnetism; secondly, the chemical history of the species; in which are to be considered its nature, as elemental or compound, its chemical relations to other species, and these relations as modified by physical conditions and forces. The quantitative relation of one mineral (chemical) species to another, is its equivalent weight, and the chemical species, until it attains to individuality in the crystal, is essentially quantitative.

It is from all the above data, which would include the whole physical and chemical history of inorganic bodies, that a natural system of mineralogical classification is to be built up. Their application may be illustrated by a few points drawn from the history of certain natural families.

The variable relations to space of the empirical equivalents of non-gaseous species, or in other words, the varying equivalent volume, (obtained by dividing their empirical equivalent weights by the specific gravity,) shows that there exist in different species very unlike degrees of condensation. At the same time we are led to the conclusion that the molecular constitution of gems, spars, and ores, is such that those bodies must be represented by formulas not less complex, and with equivalent weights far more

elevated than those usually assigned to the polycyanids, the alkaloids, and the proximate principles of plants. To similar conclusions, conduce also the researches on the specific heat of compounds.

There probably exists between the true equivalent weights of non-gaseous species, and their densities, a relation as simple as that between the equivalent weights of gaseous species and their specific gravities. The gas, or vapor of a volatile body constitutes a species distinct from the same body in its liquid or solid state; the chemical formula of the latter being some multiple of the first, and the liquid and solid species themselves, often constituting two distinct species, of different equivalent weights. In the case of analogous volatile compounds, as the hydrocarbons and their derivatives, the equivalent weights of the liquid or solid species approximate to a constant quantity, so that the densities of those species, in the case of homologous or related alcohols, acids, ethers and glycerids, are subject to no great variation. These non-gaseous species are generated by the chemical union, or identification, of a number of volumes or equivalents of the gaseous species, which varies inversely with the density of these species. It follows from this, that the equivalent weights of the liquid and solid alcohols and fats must be so high as to be a common measure of the vapor-equivalents of all the bodies belonging to these series. The empirical formula, $C_{114}H_{110}C_{12}$, which is the lowest one representing the tristearic glycerid, ordinary stearine, is probably far from representing the true equivalent weight of this fat in its liquid or solid state; and if it should hereafter be found that its density corresponds to six times the above formula, it would follow that liquid acetic acid, whose density differs but slightly from that of fused stearine, must have a formula and an equivalent weight about one hundred times that which we deduce from the density of acetic acid vapor, $C_4H_4O_4$.

Starting from these high equivalent weights of liquid and solid hydrocarbonaceous species, and their correspondingly complex formulas, we become prepared to admit that other orders of mineral species, such as oxyds, silicates, carbonates, and sulphids, have formulas and equivalent weights corresponding to their still higher densities, and we proceed to apply to these bodies the laws of substitution, homology, and polymerism, which have so long been recognized in the chemical study of the members of the hydrocarbon series. The formulas thus deduced for the native silicates and carbon-spars show that these polybasic salts may contain many atoms of different bases, and their frequently complex and varying constitution is thus rendered intelligible. In the application of the principle of chemical ho-

mology, we find a ready and natural explanation of those variations, within certain limits, occasionally met with in the composition of certain crystalline silicates, sulphids, etc., from which some have conjectured the existence of a deviation from the law of definite proportions, in what is only an expression of that law in a higher form.

The principle of polymerism is exemplified in related mineral species, such as meionite and zoisite, dipyre and jadeite, hornblende and pyroxene, calcite and aragonite, opal and quartz, in the zircons of different densities, and in the various forms of titanic acid and of carbon, whose relations become at once intelligible if we adopt for these species high equivalent weights and complex molecules. The hardness of these isomeric or allotropic species, and their indifference to chemical reagents, increases with their condensation, or in other words, varies inversely as their empirical equivalent volumes; so that we here find a direct relation between chemical and physical properties.

It is in these high chemical equivalents of the species, and in certain ingenious, but arbitrary assumptions of numbers, that is to be found an explanation of the results obtained by Playfair and Joule, in comparing the volumes of various solid species with that of ice; whose constitution they assume to be represented by HO, instead of a high multiple of this formula. The recent ingenious but fallacious speculations of Dr. Macvicar, who has arbitrarily assumed comparatively high equivalent weights for mineral species, and has then endeavored, by conjectures as to the architecture of crystalline molecules, to establish relations between his complex formulas and the regular solids of geometry, are curious, but unsuccessful attempts to solve some of the problems whose significance I have here endeavored to set forth. I am convinced that no geometrical groupings of atoms, such as are imagined by Macvicar, and by Gaudin, can ever give us an insight into the way in which nature builds up her units, by interpenetration and identification, and not juxtaposition of the chemical elements.

None of the above points are presented as new, though they are all, I believe, original with myself, and have been, from time to time brought forward, and maintained, with numerous illustrations, chiefly in the American Journal of Science, since March, 1853, when my paper on the Theory of Chemical Changes and Equivalent Volumes, was there published. I have, however, thought it well to present these views in a connected form, as exemplifying my notion of some of the principles which must form the basis of a true mineralogical classification.

Art. XXV.—*The Repsold Portable Vertical Circle;* by Cleveland Abbe.

The progress of practical astronomy in the United States has already been distinguished by the suggestion of quite new ideas, as well as by improvements upon methods and instruments in use in Europe. It seems that a part of our national mission is to give a full and free development to whatever of good can be transplanted here from abroad; it is therefore unpardonable in us to neglect any opportunity of acquainting ourselves with the results of the experience of the astronomers of the Eastern hemisphere. The history of the brilliant life of F. G. W. Struve, to whom the world is indebted for the observatories of Dorpat and Poulkova, is doubtless familiar to all. The school of practical astronomy and geodesy that grew up under him at these two places, and is now officially established at the Central Observatory for the benefit of the Imperial Military Academy and other departments of the government, has, by the extent of the astronomical and geodesical works executed, made its influence felt far beyond the dominions of the Russian Czar. A residence of nearly two years at this Observatory has impressed the writer most deeply with the correctness of that general opinion, which for years has instinctively pointed to this magnificent institution as the head-quarters of the practical astronomy of the present day.

The extent of the territory of the United States, and the oft-recurring demand for accurate topographical maps, will increase the interest with which we study the levels, barometers, base apparatus, universal instruments, vertical circles, prime vertical and extra-meridional transits, with which the Russian astronomers have sought to meet the demands made upon them. With them, as with us, celerity is of equal importance with accuracy. The extent of their territory must forbid them, as that of ours does us, from contemplating a minute triangulation of its entire superficies—such as the smaller and more densely populated territories of the British Isles and the central European states both allowed and demanded. Our national government has rightly apprehended the importance of having the most accurate charts possible to be made of our extended Eastern and Western borders; of similar importance is the survey of our inland fresh-water lakes, now in the hands of the engineers of the War Department; of great value also is the accurate survey of international boundary lines,—but the general survey and mapping of the interior presents a problem not dissimilar from that which is being solved by the Russian geographers for their own land.

It was early seen that if astronomical determinations of *relative*

position could be made accurate to within one or two seconds of arc, the central points of reference being referred with much greater accuracy to each other and to a very few zero points, then would these relative positions, as derived from astronomical observations combined with an accurate knowledge of the figure of that portion of the earth's surface covered by these stations, suffice as groundwork for supplying the present wants of geographers and topographers. Inspired by the magnitude of the work, and supported by an interested military government, Struve and Tenner, co-working with the Norwegian and Swedish governments, carried out the astronomical and geodetical work recorded in the "Arc du Méridien de 25° 20′ entre le Danube et la mér Glaciale"—at present under the authority of O. Struve and General Baeyer, co-working with Great Britain and Belgium; the field operations connected with the measurement of the arc of longitude between Valentia and Orsk are being rapidly pushed forward and will be finished in the summer of the present year. These two great works, and the similar ones that may be expected to follow in future years, when the surveys of the immense regions of Asiatic Russia come to be connected with the surveys now being carried on by the British government, furnish the necessary determination of the figure of the earth for that portion of the globe: they find their counterparts in the geodetic astronomic works in progress or already executed upon our Atlantic and Pacific sea-board, which will afford us determinations of arcs of latitude between the parallels of 26° and 48° north, and ought to be extended to the measurement of arcs of longitude of 60° on our northern, and 40° on our southern boundaries, or possibly one of 45° between Washington and San Francisco. Up to the present decade it must be conceded that the attention of geodesists has been perhaps too exclusively directed to the measurements of degrees of latitude; it is now become important to determine also arcs of longitude, and the present European international undertaking is one worthy of emulation. It is indeed with peculiar pleasure that we notice the comparatively slight expense that would attend the junction of the present and proposed triangulations of the lake survey and of the coast survey, by a triangulation from Buffalo to Albany, leading thereby to the measurement of an arc of 18° on the parallel of 42° north between Chicago and the extremity of Cape Cod. At some future time the junction of the northwest end of Lake Superior and Cape Breton will become equally feasible, whence will result an arc of 33° on the parallel of 46° north. By the junction of the coast survey operations on the gulf of Mexico with the Pacific coast, taking advantage of the labors performed by the Mexican Boundary Survey we may be led to an arc of 33° on the parallel of 31° north, and the continuation

westward of the survey of the lakes, or rather the completion of the labors of N. W. boundary survey should lead to the determination of an arc of 55 to 60 degrees of longitude—the largest probably that will ever be measured on this continent. In former years the difficulties in the way of accurate longitude determinations may well have prevented such undertakings as those here suggested,—but at present the telegraph and chronograph and the use of accurate extra-meridional transits have undoubtedly removed those obstacles; as regards latitudes, it is probable that the Repsold portable vertical circle will long suffice for measurement of vertical angles. It is, however, imperatively necessary in determinations of longitude, that not only the observers be exchanged, but also with them their transits, their relay batteries, chronometers and chronographs, and all apparatus used at either end.

The quadrangular area of the United States (whose natural nucleus is probably found in St. Louis or Omaha City—even as for the present state of population Cincinnati may be regarded as a central point), offers the same variety of hills and mountains, plains and plateaus as is found in Russia, and by its extent requires that the curvature of its surface be determined independently of the investigations made in the eastern hemisphere. Until this is done, the topographical surveys made by the land commissioners and surveyors of the Federal government, and by the several states, ought to be considered as plane table sheets whose fundamental points (the secondary points of a triangulation), can only be properly fixed by geodetic and astronomic measurements.

The accurate, convenient, speedy and economical determination of the positions of as many of these *secondary* stations as are needed for topographical maps that do not pretend to a pedantic accuracy is the present problem;—and assuming that the coast survey will give us a sufficient knowledge of the curvature of our portion of North America, we shall arrive at a solution of our present problem by selecting central or primary astronomical stations at convenient points—for instance, one to three in each of the states east of the Mississippi, and traversed by several railroads or navigable rivers. The astronomical latitude of these primary points, and their longitudes relative to each other and to the zero point—Washington Observatory—are to be determined with all attainable accuracy. Expeditions starting from one such point (and consisting of one observer, one vertical circle, five to twenty-five chronometers, including one non- and one over-compensated, one barometer, &c.), visiting in the course of five to twenty-five days five, ten, twenty secondary stations, and returning to the same or another primary station, will be able to furnish the relative position of all the secondary points visited

to within two seconds of arc; or to be more definite, to within $0''{\cdot}5$ in latitude and $0^s{\cdot}1$ in longitude, and to give equally approximate determinations of the relative vertical heights. That this is practically done by the Russian geodesists (the first expedition dates 1846), and that in our own easily traversed country it can be better done than in theirs, is sufficient reason for calling attention to the work of Colonel Smyssloff, mentioned below, where "are fully detailed the different astronomico-geographical methods of determining position, which, by the influence of the Poulkova Observatory, have been introduced into the geodetic work of the Russian empire, and which with perfect success supplant the far more tedious and costly triangulation."*

In the wilds of Central Asia and Siberia, as in some portions of our own territory, preliminary surveys based on observations made with the pocket chronometer and Pistor and Martin's patent sextant, may supply our present need of information; but eastward to the Ural and Caucasus, it has been found practicable to transport the Repsold vertical circle and the Brauer's extra-meridional transit. The methods of using these two instruments are fully given in the two following publications:

Repsoldov Krug, Chronometri, Chronometretscheska Expedaitsir, 1859, goda. P. Smyssloff, S. Peterbourg, 1863.

Die Zeitbestimmung, vermittelst des Tragbaren Durchgangs Instruments im Verticale des Polarsterns, von W. Döllen, St. Petersburg, 1863.

A third memoir by Colonel Smyssloff (now Director of the Observatory at Wilna),

Opuity dla sravnayteljnoi otsjenkay raslaytschnech sposoboff telegraphay-tscheskoi peredatschay vraymayne pray opredjelaynie rasnoste dolgote Poulkovskoi ay Moskovskoi Observatorie. P. Smyssloff, St. Petersbourg, 1865,

gives the details of the latest and telegraphic determination of the difference of longitude between the observatories of Poulkova and Moscow. This determination was made previous to the completion of the new portable transits by Brauer, and is valuable on account of the comparison of the three methods of communicating and observing the telegraphic signals.

The excellencies of the method given in the above quoted memoir by Mr. Döllen have during the past three years found confirmation in the observations conducted by the military officers studying with him. It merits a wide circulation in our own land, which I hope to secure by a published translation with appropriate tables; questions of secondary importance seem to have prevented its exclusive adoption in the longitude determinations needed for the arc of 68° 54′ on the parallel of 52° north latitude. The surprising reliability of the portable transits made by Mr. Brauer, the mechanic of the Central Observatory, now independently established in St. Petersburg, has shown them to be adapted to the highest requirements of the present state of

* Otto Struve, Jahresbericht, June 14, 1863.

geodesy, whilst the great saving of time when they are used in the vertical of Polaris gives these instruments a remarkable superiority over the meridian transit, affording a full confirmation of the thesis propounded on page 13 of the above named memoir by Mr. Döllen, "under all circumstances, the determination of time for any given instant will be best made by mounting the portable transit, not in the plane of the meridian, but in the vertical of the pole star." Mr. Brauer has intimated to me his intention to place his sixth transit on exhibition at the Paris Exposition. Nor can I here refrain from expressing my conviction of the great importance to the interests of our astronomy and surveying to be attached to the establishment in this country of a mechanical institute, which, under the charge of a person of the experience of Brauer and the Repsolds, shall be able to furnish us with measuring instruments comparable with the objectives produced by our opticians, and fitted to do the fine work in which the astronomer and geodesist are so much interested.

For the determination of latitude the same portable transit, by being established in the plane of the prime vertical, yields zenith distances whose accuracy is in general much greater than that of the declinations of the stars observed, since these are in general faint ones. The portable prime vertical transit loses therefore somewhat of its importance, excepting for determinations of latitude where the highest accuracy is sought, and when by the coöperation of fixed observatories, special simultaneous investigations into the declinations of the stars observed can be obtained.

In general, the use of the prime vertical transit restricts one to the observation of a limited number of faint stars situated in a narrow belt. The use of the Talcott zenith telescope demands more accurate declinations of faint stars than are generally accessible, and the increased size of the telescope as well as the time required for making a large number of observations on favorable nights, constitute objections to the use of both those ingenious methods in latitudes greater than 45° (Poulkova is in latitude 60° N.), where the period favorable for field operations is comparatively short, and distinguished by long twilight.

The observations of azimuths or of zenith distances are adapted to the determination of time and latitude; and if we adopt the principle that a small number of accurate observations is preferable to a larger number of less accurate ones, it becomes necessary to restrict our attention to the three or four hundred brighter stars whose present places are known with considerable accuracy. The portable vertical circle offers itself to us as an instrument equally applicable in all latitudes to the determination of latitude, and fit for the determination of time for secondary stations between 70° of latitude and the equator, if accurately constructed as by Repsold, and used so as to eliminate

constant or systematic errors. By reason of the ease with which it is put in position, and the brightness of the stars observed, as well as by the accuracy of its divided circles, level and microscopes, there is no time lost nor money expended in building stations, nor in waiting for nightfall, nor in tedious repetition of observations. Some of the principles embodied in the Repsold construction of this instrument may be found introduced into other instruments previously constructed for Struve by these artists, by Ertel, by Brauer, &c.; but the first of those on the perfected pattern now adopted was constructed in 1851, from designs furnished† by His Excellency, Otto Struve, the present Director of the Central Observatory. This one belonged to the topographical staff of the Imperial Military Academy, and was destined to be used in the surveys of the Caucasus; it could therefore only be once used in the new revision of the latitudes of the thirteen principal points of the Russian-Scandinavian arc; the results for that one station, Kilpi Mäki, 1852, afforded, however, a very satisfactory proof of the quality of the instrument. The considerable number of these instruments already made by the Messrs. Repsold, (and especially the fine one ordered for the hydrographical staff, and into which were introduced a number of minor improvements suggested by Mr. Brauer), have by their continual use and their successive improvement, led to the belief that the vertical circle has as yet but begun its course of usefulness, and will, with further improvements, eventually be entirely depended on for doing the work that it is so admirably fitted to accomplish,—and farther, that the principles carried out in its construction, i. e., compactness, high magnifying powers, reversibility, &c., have received authoritative confirmation as to their correctness.

The first of the memoirs of Colonel Smyssloff above quoted gives a detailed description of the instruments used and the work done in a chronometric expedition carried out by himself in 1859, in the neighborhood of Poulkova, in which seventeen points were accurately determined between the 18th of June and the 31st of July. We shall here give a brief account of the vertical circle used and of the plan of the expedition.

The aperture of the telescope used was $1\frac{1}{8}$ inches with a focal length of 20 inches—these dimensions, especially the aperture, have in later instruments been somewhat increased. The conical tube holding the objective is $9\frac{1}{2}$ inches long, being screwed to the cube containing the prism of total reflection whose center is 5 inches distant from the two Ys in which the pivots of the axis rest, and $9\frac{1}{2}$ inches distant from that end of the steel axis that carries the two parallel wires midway, between which the observed star is to be brought. The opposite end of the axis being perforated admits light for the illumination of the field.

† See Arc du Meridien. Introd., p. xxxviii.

The bisection of a star by a single wire presents serious disadvantages in attempting observations in strong twilight on faint stars, and is not generally attempted. The wires are stretched across the end of a cylindrical tube, whose independent connection with the axis isolates it as far as possible from the exterior and protecting cylinder carrying the ocular. On either side of the cube and clamped to it by outer plates are the two circles having each 8 spokes and divided silver arcs of 11 inches diameter.

The supporting arms, whose extremities form the Y's, branch from a conical column 6 inches high; this is hollow, and within is the vertical axis on which the reversion takes place from circle and observer east to circle and observer west, or *vice versa*. This vertical axis rests upon three horizontal legs through whose extremities pass the foot screws with divided heads. The distance from the foôt screws to the center axis of the instrument is about 7 inches; the whole height from the ground to objective when pointed to the zenith being two feet and two inches. The weight of the instrument is probably about forty pounds.

A horizontal finding circle with two verniers and slow-motion screw is attached to the vertical column just above the plane of the horizontal legs. The vertical circle on the side of the cube opposite to the ocular is used as a finding circle and for giving the slow vertical motion; it is provided with two pointers at the opposite ends of a horizontal bar which rests upon the horizontal axis of revolution just within the pivot, and which is held in position by a suspended vertical frame held by an adjusting screw six inches below the axis. A similar horizontal bar similarly placed near the other pivot carries the level and the two reading microscopes which are perpendicular to the plane of the circle.

In observing, therefore, the eye being directed through the telescope ocular, one has the microscopes on the left and right in a horizontal line, distant from the eye some 5 or 6 inches, whilst the level is directly in front, which convenient compactness greatly facilitates the observations.

The heads of the micrometer screws of the microscopes are divided into 60 parts each representing 2″; the level divisions have the same value. The microscopes are about 6 inches long, having single lenses of aperture 0·2 inches for their objectives, and a magnifying power of 20–25 diameters, that of the telescope being 60–65 diameters. In the present construction of the microscopes, following a suggestion of Mr. Marth in his article on the Greenwich Transit Circle, each micrometer screw is made to carry two pairs of parallel wires, the centers of the pairs being distant $1\frac{1}{2}$ revolutions of the screw from each other. The image of the divided limb, which is formed in the plane of the micrometer threads, is enlarged about 3·5 times, corresponding therefore to a circle whose radius is the same as the focal length of the telescope objective.

By comparing these dimensions with those of the four times larger Ertel vertical circle of the Central Observatory, it is evident that the portable instrument, for which the actual probable accidental error of a meridian zenith distance resulting from two pointings, one circle east and one circle west, is $\pm 0''.5$, has derived a decided advantage from its small size and proportionately higher magnifying power.

Recognizing the principle that in an astronomical measurement the fewest possible assumptions must be made,—and that computed probable errors give a very unreliable or perhaps no indication of the extent to which the constant or systematic errors introduced by assumptions as to the condition of the instrument, &c., may have vitiated our results,—it is necessary not only to examine thoroughly the instrument itself, but also in using it to still farther reduce the influence of its imperfections. Therefore a zenith distance is made to depend upon eight pointings of the telescope (or 4×8 pointings of the microscope micrometers), the four in one position of the circle being preceded and succeeded by two in the opposite position; the whole series requiring from sixteen to twenty-four minutes for its complete observation. A latitude or a time determination depends upon a pair of stars observed on opposite sides of the zenith at the same zenith distance; or upon sixteen pointings whose result is sensibly free from any assumption as to the zenith point, flexure or refraction.

The examination of the instrument used by Colonel Smyssloff leads to the following results.

The error of bisecting the interval between the two wires of either pair in the field of view of the microscope by a division of the limb of the circle, the error arising from accidental errors of the screw and the divisions of the screw head, and the error of reading these divisions, combine to affect the mean of four measurements with the microscopes, $\pm 0''.17$

The probable accidental error of a division of the divided circle, $\pm 0''.46$, affects the mean of four, $\pm 0''.23$

The probable accidental error of a reading of the two ends of the level bulb, $\pm 0''.12$

The combination of these gives

$$\sqrt{(0''.17)^2+(0''.23)^2+(0''.12)^2}=\pm 0''.31.$$

The probable accidental error of pointing on a star may be afterward investigated,—but if we assume that it equals an apparent visual angle of 1′, this will correspond to an arc of $\pm 0''.67$. Whence

$$\sqrt{(0''.31)^2+(0''.67)^2}=\pm 0''.74.$$

A zenith distance depending on one pointing, circle right, and

one, circle left, may therefore be expected to be affected with an accidental error of

$$\frac{\pm 0''.74}{\sqrt{2}} = \pm 0''.52;$$

and for one depending on eight pointings we have a probable accidental error, $\pm 0''.26$. A latitude or time determination depending on two such zenith distances has accordingly the probable accidental error of observation, $\pm 0''.18$.

The influences of refraction, clock correction, flexure, periodic errors of division, still remain. The latter have not, to my knowledge, as yet been specially investigated,—the circles are divided with the same machine used in dividing the small circles investigated by Struve in Dorpat. See his "Beschreibung der Breite Gradmessung," and the "Description de l'Observatoire Central." The combined influence of all disturbing causes can be investigated by a series of determinations of the latitude of any known station,—the zenith point of the divided circle being successively altered by arcs of 30° or 45°. Sixteen determinations of the latitude of Poulkova afford an example of this investigation which should be entered into by each observer for his own instrument. Using the declinations given in the British Nautical Almanac, there results the following series of values of the latitude of the station, which was the northeast small dome on the grounds at Poulkova, and whose latitude, by reference to that of the Ertel vertical circle as deduced by Dr. Peters, is $+59°\ 46'\ 20''.02$. Each of the following values of φ results from one observation consisting of eight pointings upon the respective stars.

Zenith point of the circle —0°.

	Star observed.	Meridian zen. dist.	Resulting latitude.	Means by pairs.
			59° 46′	
1.	β Urs. Minoris	−14° 58′	18″.77	19″.89
	α Cygni	+15 0	21 .01	
2.	β Urs. Min.	−14 58	18 .64	19 .55
	α Aurigæ	+13 55	20 .46	
3.	Polaris	−31 41	20 .27	20 .43
	α Coronæ	+32 35	20 .59	
4.	Polaris	−31 41	19 .72	19 .98
	α Coronæ	+32 35	20 .24	
5.	β Urs. Min. s. p.	−45 30	21 .66	20 .50
	α Tauri	+43 33	19 .35	
6.	β Urs. Min. s. p.	−45 30	21 .44	20 .46
	α Tauri	+43 33	19 .47	
7.	α Aurigæ s. p.	−74 23	21 .92	20 .53
	α Virginis	+70 12	19 .14	
8.	α Aurigæ s. p.	−74 23	21 .46	19 .88
	α Virginis	+70 12	18 .29	

Zenith point of the circle —90°.

	Star observed.	Meridian zen. dist.	Resulting latitude.	Means by pairs.
			59° 46′	
9.	β Urs. Min.	−14° 58′	19″.54	20″.86
	α Cygni	+15 0	22 .19	
10.	β Urs. Min.	−14 58	18 .73	18 .98
	α Aurigæ	+13 55	19 .24	
11.	Polaris	−31 41	20 .31	20 .04
	α Coronæ	+32 35	19 .78	
12.	Polaris	−31 41	19 .13	19 .34
	α Coronæ	+32 35	19 .54	
13.	β Urs. Min. s. p.	−45 30	22 .24	20 .88
	α Tauri	+43 33	19 .51	
14.	β Urs. Min. s. p.	−45 30	21 .48	20 .24
	α Tauri	+43 33	19 .01	
15.	α Persei s. p.	−70 52	22 .98	20 .15
	α Virginis	+70 12	17 .32	
16.	α Persei s. p.	−70 52	21 .73	20 .20
	α Virginis	+70 12	18 .66	

Mean of 8 pairs, 20 .15

" 16 " 20 .08

By reference to the Ertel circle, +59° 46′ 20″.12
59 46 20 .02

Difference, 0 .10

Assuming the 16 values resulting from the 16 pairs to be free from flexure, there results a probable error of latitude from one pair $= \pm 0''.35$.

The difference of the latitudes resulting from the two stars of each pair depends upon the flexure of the tube and the error in the declinations, as well as upon any systematic error in the refraction or the graduation, though this is probably insensible. Assuming the latitude to be 59° 46′ 20″·00, we find the differences from this to be represented by the formula

$$(A.) \qquad +3''.16 \sin (z - 30°.3);$$

applying this to each of the thirty-two observations, there results a probable error of a latitude from two stars $\pm 0''.34$. From the sixteen values of the flexures we derive a probable error resulting from the error in the ephemeris and the error of pointing and reading; this is $\pm 0''.31$. And subtracting the latter source of error, there results $\pm 0''.25$ as the probable error of the declination in the British Nautical Almanac.

The investigation of flexure might also be made by means of observations in the prime vertical, but here we probably have a complicated combination of flexure and personal equation. Eight determinations of time made by Colonel Smyssloff (each depending upon eight pointings on each of a pair of stars observed in the prime vertical), compared with simultaneous observations by Wagner at the Ertel transit gave

$$\text{Wagner–Smyssloff} = -0^s.02 \pm 0^s.03;$$

the probable error of a single determination resulting $= \pm 0^s.09$; or, if we allow equal accuracy to each instrument, the probable error of a clock correction given by the vertical circle $= \pm 0^s.06$.

A series of comparisons between Messrs. Smyssloff, Bolscheff and Demetrieff, in which each observed four of the eight pointings gave,

$$\text{S.–D.} = -0^s.098, \quad \text{S.–B.} = -0^s.046, \quad \text{B.–D.} = +0^s.132;$$

and the probable error of a determination of time $= \pm 0^s.06$, which in the latitude 60° corresponds to a vertical angle of $\pm 0''.45$.

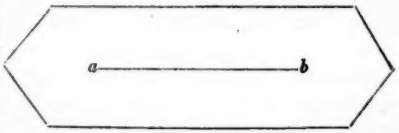
As in the determination of latitude so in that of time, a pair of stars equally distant from the zenith is always observed, each being pointed upon four times in each position of the circle, the eight pointings requiring twenty minutes or less. The stars are of course observed near the prime vertical.

[To be concluded.]

ART. XXVI.—*On Cryophyllite, a new mineral species of the Mica Family, with some associated minerals in the granite of Rockport, Massachusetts;* by JOSIAH P. COOKE, Jr.

IN a paper published in a recent number of this Journal* I described a new mineral species allied to Helvin, to which I gave the name of Danalite. Associated with Danalite in the granite ledges forming the extremity of Cape Ann, Massachusetts, are two remarkable micas, one of which is the new species to which on account of its easy fusibility and foliated structure I have given the name Cryophyllite.

Mineralogical Characters.—The mineralogical characters of cryophyllite are as follows. Like other varieties of mica it crystallizes in six-sided prisms, which are frequently of considerable size, from one to two inches in length and of proportionate diameter. The basal cleavage is highly perfect, yielding thin flexible and transparent laminæ, which when examined with a polarizing microscope give a biaxial image, the angle between the optical axes varying from 55° to 60°. The angles 55°, 57° 30′, and 60°, were all measured on different specimens. The dispersion of the axes, if any, is so slight that its character could not with certainty be determined, the color of the mineral tending to obscure any such effect. The perfect uniformity of the two systems of rings both as to form and to color indicates that the mica belongs to the trimetric system. Nevertheless, the angle between the prismatic planes measured with an application goniometer 120° as accurately as is possible with this instrument, and in one instance the planes of an hexagonal pyramid terminating the prism were distinctly seen. Considering, however, these crystals as belonging to the trimetric system, in accordance with the generally received opinion of mineralogists as to crystals of the mica family, all of which present essentially the same crystallographic characters, we must regard the six-sided prism of cryophyllite as formed by the union of the planes *I* of the rhombic prism with the brachydiagonal basal planes $i\breve{i}$. The plane of the optical axes coincides with the shorter diagonal of the rhomb base, and the crystals are frequently much elongated in this direction, so that the form of the cleavage face was as shown in the accompanying figure, the line *a b* indicating the position of the plane of the optical axes. Furthermore, the crystals were frequently twinned together on the plane $i\breve{i}$, and it was observed that in such cases the planes



* Vol. xlii, No. 124, July, 1866.

of cleavage of the two crystals were absolutely coincident, proving that the rhombic prisms are rectangular and not oblique, as De Senarmont has previously shown to be true of other crystals of the mica family.

The color of cryophyllite in axial directions is dull emerald green, not unlike glass colored with protoxyd of iron, and so deep that the laminæ are opaque unless quite thin, but like other colored micas it is dichrous and appears brownish-red in the direction of the lateral axes. The color of the streak is light gray with a tint of green. The luster is brilliant on the cleavage face inclining to resinous. The hardness is from 2 to 2·5 and the specific gravity 2·909.

Before the blowpipe cryophyllite very easily fuses with some intumescence to a greyish enamel bead, and it even fuses in flakes of considerable size in the flame of a candle, so that its fusibility is from 1·5 to 2 of von Kobell's scale. It imparts to the flame of a Bunsen's lamp a most brilliant lithia reaction, and by examining the colored flame with a spectroscope the presence of potassium, sodium and rubidium may also be readily discovered. The amount of sodium however must be exceedingly small as it does not sensibly modify the lithia flame as seen by the naked eye, and the same is true to a still greater degree of rubidium. The presence of rubidium is best recognized by mixing the pulverized mineral with pure pulverized sulphate of lime, exposing a small bead of this mixture supported by a loop of fine platinum wire to the flame of a gas blowpipe and examining the flame with a spectroscope. The characteristic double blue line of rubidium is then seen very distinctly for a few moments, but soon disappears. No trace of cæsium could be discovered, either by the mode of experimenting just described or by examining the platinum salt obtained in the course of the analysis, by the partial precipitation of the alkalies with chlorid of platinum.

Heated alone in a closed glass tube cryophyllite slightly changes color, but gives no sublimate, although when heated in the same with bisulphate of potash it gives a strong reaction of fluorine. When in fine powder, it is completely decomposed after some time even by the dilute mineral acids, the silica separating as a fine powder. From this description it is evident that in its mineralogical characters cryophyllite closely resembles other members of the mica family, especially the lepidolites, differing from these chiefly in the ease with which it is decomposed by acids and in a somewhat greater fusibility.

Method of Analysis.—In analyzing the mineral the fine powder was decomposed in a closed glass flask in the same way as described in my paper on Danalite, using dilute hydrochloric or sulphuric acids as the case required. Each complete analysis was made with three portions of the same powder. From the first

portion, decomposed by hydrochloric acid, the silica, manganese, iron and aluminum were determined, the last two being precipitated as basic acetates and weighed together as oxyds, and the manganese subsequently precipitated from the filtrate by bromine. From the second portion, decomposed by dilute sulphuric acid, with perfect exclusion of air, the amounts of protoxyd and sesquioxyd of iron were determined by means of permanganate of potash, when by subtracting the total quantity of iron, calculated as sesquioxyd, from the sum of the alumina and sesquioxyd of iron already known, the amount of alumina was ascertained. From the third portion of the powder, decomposed by hydrochloric acid, the magnesia and the alkalies were determined. The alkalies were weighed as sulphates, and from the amount of sulphuric acid subsequently determined, the relative quantities of potash and lithia were calculated. From the fourth portion of the powder the quantity of fluorid of silicon was determined. In the first analyses of the mineral the fluorine was precipitated as fluorid of calcium according to the well known method of Berzelius as modified by Rose; but the process was found to be very tedious and the results not accordant. Subsequently the following method was devised, which may be found to be useful in other cases.

Determination of Fluorid of Silicon.—A weighed amount of finely pulverized mineral is first decomposed in a tared platinum dish with dilute sulphuric acid, and the greater part of the water having been removed on a steam bath and the excess of sulphuric acid having been driven off by gradual heating on a sand bath, the whole mass is ignited during at least an hour, the dish meanwhile being covered with a piece of platinum foil forming part of the tare. The dish with its contents having been weighed, the dried mass is next treated with hydrochloric acid and the residual silica determined in the usual way, and in the filtrate from the silica the sulphuric acid is determined and weighed as sulphate of baryta. We thus obtain four weights: A, the weight of the mineral employed in the analysis; B, the weight of the ignited residue; C, the weight of the residual silica; and D, the weight of the residual sulphuric acid. It is now evident that A+D−B equals the weight of the fluorid of silicon, and that B−C−D equals the weight of the sum of all the bases present. The iron is of course present in the residue as sesquioxyd, but in order to ensure its complete oxydation, it is important to add a few drops of nitric acid to the decomposed mineral, before expelling the excess of sulphuric acid. The sum of the weights of all the bases, which we find incidentally as one of the results of this method, not only furnishes an important control of the rest of our analysis, but also enables us to test the accuracy of our method. In the analysis given below it appears that the

sum of the bases each separately determined as the mean of two analyses equals 45·03 per cent, while the sum of the bases weighed together in the method just described equals 45·18 per cent, as the mean of three determinations. A closer agreement than this could not possibly be expected. The process is comparatively short and presents no unusual difficulties. The chief liability to error is in the determination of the sulphuric acid, and it is necessary in washing the sulphate of baryta to take great care to remove the impurities, which this precipitate is apt to carry down; but the methods recommended by Rose and Fresenius were found to be generally sufficient for the purpose. Nevertheless, when the amount of fluorine is less than one per cent, the old process is to be preferred.

Results of Analysis.—Two very distinct varieties of cryophyllite were analyzed,—the one finely foliated and consisting of an aggregate of small crystals and the other in large distinct crystals imbedded in massive feldspar; but no other differences of character were observed, and they were found to have the same percentage composition. The purest material was selected for analysis and such as had evidently never undergone the slightest change. There is probably in all the analyses a small loss of silica, but the error, if any, is in that direction. The results are given in the following table. In columns 1 and 2, we have the complete analyses made as first described of the large crystals, while in columns 3, 4 and 5, we have the partial analysis made of the more finely foliated variety by the process last described in connection with the determination of fluorid of silicon.

Analysis of Cryophyllite from Rockport.

	1.	2.	3.	4.	5.	Mean.	Oxygen.	
Silica,	51·53	51·54	51·65	51·37	51·36	51·49	27·46	
Fluorid of silicon,			3·29	3·34	3·62	3·42	1·06	=28·52
Alumina,	16·76	16·77				16·77	7·82	
Sesquioxyd of manganese,	0·33	0·35				0·34	0·10	
Sesquioxyd of iron,	2·00	1·94				1·97	0·59	= 8·51
Protoxyd of iron,	8·00	7·96				7·98	1·76	
Magnesia,	0·76		45·06	45·29	45·02	0·76	0·30	
Potassa,	13·14	13·16				13·15	2·23	
Lithia,	4·05	4·06				4·06	2·16	= 6·45
Soda,	*trace.*					*trace.*		
Rubidia,	"					"		14·96
						99·94		

Oxygen ratio, 14·96 : 28·52

14·26 : 28·52 = 1 : 2.

General formula, RO, SiO_2. Probable rational formula, $(\frac{1}{3}\dot{R}^3 . \frac{1}{3}\ddot{R}), 3\tilde{S}i)$.

The fluorine in the mineral is so firmly incorporated that, as stated above, it is not driven out even by a red heat, and there can be no doubt that it replaces a portion of the oxygen of the silica in the proportion of one equivalent of fluorine to every twenty-eight of oxygen. The ratio of the oxygen of the prot-

oxyds to that of the sesquioxyds is as 13 to 17, but if we estimate all the iron as protoxyd, assuming with many mineralogists that a sesquioxyd base may under certain conditions replace a protoxyd base, we shall then have very nearly a ratio of equality and may then write our general symbol $(\frac{1}{2}\dot{R}^2 . \frac{1}{2}\dot{R})_3 3\ddot{S}i$.

As has already been stated, the physical characters of the new mineral do not differ essentially from those of other well known micas, and even its greater fusibility is shared with some varieties of lepidolite, resulting probably from the very large amount of alkali which the silicate contains. The ground on which this mica must be regarded as a new species is the definite oxygen ratio of 1 to 2, which has never before been observed among the micas. The five analyses given above of two distinct varieties, from different localities, differing markedly in appearance, leave no doubt in regard to the definiteness of this ratio, and no more specific character for a new species than this could be desired. When now we consider the very great want of definiteness in the micas as a class, their composition varying between $2RO, SiO_2$ in several varieties, and RO, SiO_2 in cryophyllite, it would appear that the new mica has a distinct typical character, forming as it were one boundary of this family of minerals. Moreover, it is at least a probable theory that the two formulæ just given represent the only two distinct types of micas, and that the variation of composition may result from an admixture of these two isomorphous species. The masterly investigations of De Senarmont on the optical relations of the micas tend to show that such an admixture exists, and at least prove that even a very large difference in the angle of the optical axes does not afford a safe ground for making a distinction in species. He supposed that this difference arises from a mixture of two micas crystallographically isomorphous but optically distinct, the one, and the more common class, having the optical axes in the plane of the longer diagonal, while in the other they fall in the plane of the shorter diagonal, as is the case with cryophyllite. Moreover, he was enabled, by crystallizing together certain isomorphous salts, to produce artificially the same result, which we have naturally in the micas. If then the variation of optical characters results from an isomorphous mixture of *optically* distinct species, it is at least probable that the variation of composition, so far as the oxygen ratio is concerned, may be caused by a similar mixture of *chemically* distinct species, and this view may serve to bring into some sort of order the almost hopeless confusion which the analyses of micas have hitherto presented. The magnesian micas phlogopite and biotite are sufficiently distinguished by the character of their bases, and conform very closely to the general formula $2RO, SiO_2$; but there does not seem to be any good ground for distinguishing between

the two. The optical distinction, as we have seen, is not specific, and if sesquioxyds may replace protoxyds, any apparent difference in the relative proportions of these oxyds is equally of no value as a ground of separation. Even this last difference, however, is not constant, and biotite must be regarded as a phlogopite in which a certain amount of magnesia has been replaced by protoxyd or sesquioxyd of iron. Passing next to lepidomelane, we have a pure iron mica in which all the magnesia has been in like manner replaced. Lastly, the presence or absence of a small and very variable amount of lithia does not seem to make any good ground of distinction between lepidolite and muscovite, and the only essential difference between these species is the much greater fusibility of the former, arising probably from the large amount of alkali and fluorine entering into its composition, a quality which we have endeavored to express by the name cryophyllite, following the analogy of the name cryolite. In lepidolite the oxygen ratio varies very widely between the extreme limits already named, the variation resulting, according to our theory, from a mixture of two species, the first having the type ratio 1 : 1, which we may still call lepidolite, and the second having the ratio 1 : 2, which has been described in this paper and called cryophyllite. A similar admixture of an isomorphous species having the type ratio 1 : 2 may explain the equally wide variations in the oxygen ratio of the muscovite, although the infusible mica corresponding to cryophyllite has not yet been observed. It is manifest that the theory here advanced would be very greatly substantiated if it could be shown that cryophyllite is actually associated with a distinct but isomorphous mica having the type ratio 1 : 1, and that it does actually influence the composition of its associate in the way we have indicated. Such is the case, as we believe, at the Rockport locality.

Lepidomelane.—The mica which is associated with cryophyllite at Rockport (or as we should rather say, with which cryophyllite is associated, for cryophyllite is the subordinate species) is an iron mica of the species lepidomelane. This is the common mica of the great granite ledges which form the extremity of Cape Ann. In the granite itself, however, it occurs only in small flakes forming a very small proportion of the whole mass; but in the numerous veins which intersect the rock, lepidomelane is found in crystals of considerable size and sometimes in plates several inches in diameter. The vein, from which most of the best specimens have been taken, is an offshoot of one of the great trap dikes which cross the Cape from north to south nearly parallel to each other, and consists chiefly of massive quartz and feldspar almost completely segregated, the quartz lining the lower wall, while the feldspar lines the hanging wall

of the vein. The best crystals, both of lepidomelane and of cryophyllite, are found in the massive feldspar, sometimes in direct contact and interpenetrating each other, while at other times they are comparatively isolated. The crystals of the two minerals resemble each other so closely in their external aspect that it requires some experience to distinguish them, without applying the distinctive test soon to be mentioned, and even an experienced mineralogist might readily mistake an aggregate of the two for a homogeneous mass. Like cryophyllite the lepidomelane crystallizes in hexagonal prisms with an angle of 120° between the prismatic planes, as was measured on several specimens with an application goniometer. Moreover the crystals have the same general habitus as those of the associated mica. They are frequently seen elongated in the direction of the brachydiagonal and twinned together on the plane $i\breve{i}$, and, in a word, the description of the crystals of cryophyllite already given applies equally well to those of lepidomelane; but unfortunately on account of the great opacity of lepidomelane I have not yet succeeded in determining its optical relations. There can be no doubt, therefore, that the two micas are perfectly isomorphous. But while the outward resemblance of these micas is so striking, the mineralogical characters of the two species are wholly distinct, as will be seen from the following description.

Mineralogical characters of Rockport Lepidomelane.—The cleavage is basal and perfect, but it is not so eminent as in most micas, and the foliæ are not at all or only slightly elastic. The color is black, and that of the streak dark green, differing wholly from the color of the streak of cryophyllite, which is light gray with only a faint tinge of green. The laminæ are opaque unless exceedingly thin. The luster is very brilliant, inclining to resinous on the cleavage face. The sp. gr. was found to be 3·169, and the hardness about that of calcite, or 3. Before the blowpipe it fuses to a black enamel bead, which is highly magnetic, and this affords another ready means of distinguishing it from cryophyllite. The fusibility is about that of iron-garnet, or between 3 and 4 of von Kobell's scale. With borax it forms a bottle-green glass. The powder when heated in a closed glass tube gives off water, and its green color changes to a pinchbeck-brown not unlike that of magnetic pyrites. Heated with bisulphate of potash it gives a slight reaction for fluorine. The powder is easily decomposed by the dilute mineral acids, the silica separating in fine scales. These characters agree very closely with those of the original lepidomelane from Wermland as described by Soltmann,* only the more perfect crystals from Rockport enable us to determine the crystalline form with more cer-

* Pogg. Annalen, l, 664, also Dana's System, 4th edition, vol. ii, page 297.

tainty than was possible with the minute scales from which the species was first established.

Analysis of Rockport Lepidomelane.—My analyses of this mica are given in the following table. In the analyses 2, 3, and 4, the same methods were followed which have already been described in connection with cryophyllite. In analysis 5, I employed, with some slight modification, the method first proposed by H. Saint Claire Deville,* and the close agreement of the figures obtained by such very different methods serves to confirm the general accuracy of the result. The determination of the water was the chief difficulty met with in the analyses. The water enters so intimately into the composition of the mineral that it is not expelled below a low red heat, and then only quite slowly. Moreover at this temperature the mineral is rapidly oxydized. But by igniting the mineral in a current of carbonic acid gas the oxydation was prevented and constant results were obtained. With this precaution portions of the same powder were ignited, first without and then with oxyd of lead with the following results:

	1.	2.	3.	Mean.
Loss on ignition without PbO,	1·54	1·52	1·52	1·53
" " with "	1·55	1·40		1·48

The difference is within the limit of probable error, and the results prove that the fluorine is not expelled at a low red heat. Moreover the mineral after ignition gave as strong a reaction for fluorine as before, and when heated to the highest temperature which could be obtained with a gas blast lamp, underwent no farther loss of weight. This is in accordance with what we found to be true of cryophyllite; but not in accordance with the comportment of hydrous minerals into which fluorine enters as an essential constituent. Compare analyses of Cookeite, this Journal, [2], xli, 246. These facts, taken in connection with the circumstance that fluorine was not found by Soltmann in the lepidomelane of Wermland, lead to the inference that the fluorine in the Rockport mineral is an accidental constituent, derived from the admixture of material foreign to the species itself. The amount of fluorid of silicon in analysis 2 was determined by the method of Berzelius; but the limit of error in this process is so wide that the result can only be regarded as approximate. The analyses which follow were all made on material dried at 100°, and selected to represent the different varieties of the mineral found at Rockport. Analyses 2 and 3 were made from portions of a mass of comparatively compact material. No. 4 from a perfect crystal imbedded in the massive feldspar of the vein already referred to, and the silica determination. No. 1 from scales picked out from the granite of Boylston Hall in Cambridge, which is built of stone from the Rockport quarries.

* Annales de Chimie et de Physique, [3], xxxviii, 5.

Analysis of Rockport Lepidomelane.

	1.	2.	3.	4.	5.	6.	7.	8.	Mean.	Oxygen.	
Silica,	39·50	39·49	—	39·79	39·43	—	—	—	39·55	21·09	
Fluorid of silicon,		0·62	—	—	—	—	—	—	0·62	0·19	=21·28
Alumina,		16·72	16.41	16·90	16·87	—	—	—	16·73	7·81	
Sesquiox. of manganese,	—		0·63	0·58	0·59	—	—	—	0·60	0·18	
Sesquiox. of iron,	—	—	—	—	—	12·21	12·01	11·98	12·07	3·62	
Protox. of iron,	—	—	—	—	—	17·57	17·47	17·41	17·48	3·88	
Magnesia,	—	—	—	0·64	0·59	—	—	—	0·62	0·25	
Potassa,	—	—	—	10·68	10·63	—	—	—	10·66	1·81	
Lithia,	—	—	—	0·60	0·57	—	—	—	0·59	0·32	
Soda,	—	—	—	—	—	—	—	—	*trace.*	—	
Rubidia,	—	—	—	—	—	—	—	—	"	—	
Water,	—	—	—	1·55	1·40	1·52	1·52	1·54	1·50	1·33	=19·20
									100·42		

Oxygen ratio, 19·20 : 21·31.

After an examination of the results of the above analyses no one can doubt that the true oxygen ratio of the mineral is 1 : 1, and that the general formula is $2\dot{R}O, SiO_2$ or $2(\frac{1}{4}\dot{R}^3 . \frac{1}{4}\dot{R})3\ddot{S}i$. This is not only the nearest probable formula, but moreover it harmonizes with the well established formulæ of allied species and with the results of Soltmann's analysis of lepidomelane, a mineral which our Rockport mica resembles most closely in all its characters. Nevertheless the discrepancy between the actual and the probable ratio is very great, and cannot possibly be referred to impure material or imperfect processes. As has been already stated, the material used in the fourth analysis was a portion of a perfect crystal completely isolated in a mass of feldspar, and the results of this analysis agree almost precisely with those obtained in No. 5 by an entirely different process and with wholly different material. Here then are the same unsatisfactory results, which have been obtained again and again in the analyses of the micas, and have made it so difficult to reduce to order this important family of minerals. Fortunately we find at the Rockport locality what I believe to be the clue to the whole mystery. The common mica of the granite is there associated with a second mica containing twice as much silica, but still perfectly isomorphous with it in crystalline form. Now if two isomorphous salts crystallize together from the same solution, we obtain crystals which are mixtures of the two in definite proportions, the proportions depending chiefly on the relative quantity of each which may be present; but also at times on other conditions less accurately determined.* When the sedimentary rocks were undergoing the metamorphism which converted them into the granite ledges of Rockport, or when by any other means this granite was formed, the two isomorphous micas, described in this paper, did actually crystallize together, for so we find them; and it is reasonable to suppose that the same results followed in the one case as in the other, and that

* See Rammelsberg's important paper on this subject, Pogg. Ann., xci, 321.

the mica of the Rockport granite is an isomorphous mixture of these two distinct species. This conclusion, moreover, is favored by the fact that lepidomelane—the species to which this mica undoubtedly belongs—contains in other localities neither lithia nor fluorine; while on the other hand these same ingredients are strikingly characteristic of the lepidolite micas, to which cryophyllite is allied. Assuming then that the lithia and fluorine in the analyses belong to the cryophyllite and not to the lepidomelane, we have endeavored in the following table to eliminate this disturbing element from our results. Thus, in column 1 we have repeated the mean result of our analyses. In column 2 we have the amounts of the several ingredients of cryophyllite corresponding to 0·59 per cent of lithia, deduced from the analyses of this mineral given above. Subtracting these quantities from those in the first column we have the numbers in the third column. In column 4 we have reduced the same to a percentage composition, and these numbers, according to our theory, represent the true composition of the Rockport lepidomelane. In column 5 we have placed for comparison the analysis of lepidomelane by Soltmann.

Reduction of analyses of Lepidomelane.

	1.	2.	3.	4.	Oxygen.	5.	Oxygen.
Silica,	39·55	7·48	32·07	**37·39**	....=19·94	**37·40**	=19 94
Fluorid of silica,	0·62	0·50	....	....	....		
Alumina,	16·73	2·44	14·29	**16·66**	7·78	**11·60**	5·42
Sesquiox. of manganese,	0·60	0·05	0·55	**0·64**	0·19		
Sesquiox. of iron,	12·07	0·29	11·78	**13·74**	4·12	**27·66**	8·29
Protoxyd of iron,	17·48	1·16	16·32	**19·03**	4·23	**12·43**	2·76
Magnesia,	0·62	0·11	0·51	**0·59**	0·24	**0·26**	0·10
Potassa,	10·66	1·91	8·75	**10·20**	1·73	**9·20**	1·61
Lithia,	0·59	0·59	....	....	....		
Water,	1·50	....	1·50	**1·75**	1·56=19·85	**0·60**	0·53=18·71
				100·00		**99·15**	

Oxygen ratio, 19·85 : 19·94 = 1 : 1.

General formula $2RO, SiO_2$. Probable rational formula, $2(\frac{1}{4}\dot{R}^3, \frac{1}{4}\ddot{R})3\ddot{S}i$.

The result of our calculation, as will be seen, is most satisfactory; for not only is the oxygen ratio thus obtained exact, but also the numbers agree very closely with those of Soltmann, the only apparent discrepancies disappearing when the sum of the alumina and the oxyds of iron and manganese are compared together in the two analyses, and the difference in the distribution of the iron between the two oxyds is unimportant if we assume as before that the oxyds may replace each other. We have estimated in both analyses the amount of water among the bases for the following reasons, which seemed to us conclusive: first, because the loss of water is attended by a very marked change in the color of the mineral—from green to brown—

proving that the water must be chemically, and not merely hygroscopically, combined; secondly, because while water of crystallization would be present in a definite number of equivalents —one or more—the quantity of water in this mineral obeys no such law, and only amounts to a small fraction of a single equivalent; lastly, because the water is only driven out at a high temperature, indicating that it is in a state of intimate combination. There does not appear to be any simple relation between the amounts of the protoxyd and sesquioxyd of bases in the Rockport lepidomelane; but the general formula of the mineral may readily be reduced to that of the magnesian micas by assuming that a portion of the protoxyd of iron has been replaced by the sesquioxyd.

The facts advanced in this paper seem to show quite conclusively that the variation in composition of the Rockport lepidomelane from the normal type is caused by the admixture of a second isomorphous mica with a higher oxygen ratio, which we have called cryophyllite, and this being the case we may expect to find that similar variations in the composition of other micas are due to a similar cause, and it will therefore be interesting to search for the disturbing element at the various localities. Such an examination will be likely to reveal either the presence of cryophyllite itself or else of some new species analogous to it; and if the Rockport locality is any guide, such minerals are more likely to be found in feldspathic veins or nodules of the granite rather than uniformly diffused through the rock itself. It is a fact worthy of notice, and which is quite evident from the above analyses, that the proportions in which cryophyllite is mixed with lepidomelane are quite constant throughout the Rockport locality; and this again is wholly in accordance with the well known facts which have been developed by Rammelsberg* and others in regard to the crystallization of isomorphous salts when mixed together in the same solution. It appears from these investigations that isomorphous salts do not necessarily crystallize together in every proportion, but that, in most cases at least, any two given salts have, so to speak, a definite capacity for each other, and that when the point of saturation is passed, pure crystals of the salt in excess may be deposited in direct contact with those which are mixtures of the two. Such experiments furnish an exact counterpart of the conditions which we actually realize in the Rockport granite.

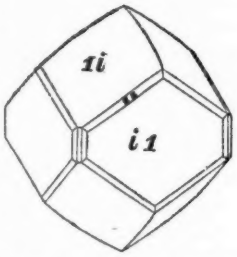
Albite.—The general nature of the vein in which the best crystals, both of cryophyllite and lepidomelane, have been found, has already been described in the earlier part of this paper. This vein consists chiefly of quartz and feldspar of the ortho-

* Pogg. Ann., vol. xci, p. 321.

clase variety. Very fine green colored crystals of the last have been obtained here, and good crystals of the usual color are abundant. Associated with the orthoclase I have frequently noticed a compact variety of albite distinguished by a peculiar luster and very marked striation on the plane of easy cleavage. It resembles somewhat, in outward aspect, oligoclase, but the mineral had the characteristic cleavages of a clinoclase feldspar, and the angle *O* on $i\bar{i}$ measured with an application goniometer 93°. Moreover it contains, if any, only the merest trace of lime. The albite is actually imbedded in the orthoclase, and the intimate association of these two heteromorphous species is worthy of notice.

Malacone.—The same vein contains also a peculiar variety of zircon, which is probably identical with the malacone of Scheerer, only somewhat more altered. The crystals have the general form represented in the figure, and resemble the crystals of zircon from Expailly in France. On account of the great predominance of the planes $1i$ and $i1$, they resemble the rhombic dodecahedron of the regular system, and can only be distinguished from this form by the distribution of the modifying planes; since on account of the strong curvature of the faces the limits of necessary uncertainty in the measurement of the angles cover the whole difference between this form and the corresponding form of zircon. In the ordinary method of representing the crystals of zircon the relation of the forms just referred to does not appear; but if we refer the planes of the zircon crystals to axes corresponding in position to those of the allied form of the regular system, the really close affinity of the two becomes evident. Thus the ratio of the axes, which in the regular system is unity, is in zircon $a:b=0{\cdot}906:1$, and the angle $1i$ on $i1$, which in the dodecahedron equals 120°, is in the zircon crystal equal to 123° 19′. In the figure here given the crystal has been drawn and the planes lettered with reference to the axes of the dodecahedron, and this must be borne in mind when comparing it with other figures of zircon. The most striking peculiarity of the crystals is the strong curvature of the terminal faces, which has been indicated in the figure, and which, as already intimated, renders accurate measurements impossible. Nevertheless, within the limits of necessary uncertainty the angles measured the same as those of zircon. The small planes not lettered on the crystal for want of space are ii and $i3$.

2.



The color both of the crystal and the powder is brownish-red,

and this color is not altered by ignition. Luster more or less adamantine on different specimens, evidently varying with the degree of alteration, and becoming more brilliant after ignition. Fracture after ignition conchoidal and very brilliant, but varying on the natural crystals, like the luster, with the degree of alteration. There is no distinct cleavage. Hardness before ignition from 5 to 5·5; after ignition from 7 to 7·5. Specific gravity before ignition from 3·985 to 4·040, the extreme limits observed; after ignition 4·095. Before the blowpipe the mineral is infusible. In a closed tube it gives off water, which has at first an alkaline reaction, but becomes acid when the mineral is heated more intensely. With bisulphate of potash gives slight reaction for fluorine. The powder dissolves with some difficulty in melted borax, giving only the reaction for iron. In phosphorus salt it dissolves only with great difficulty and incompletely, giving in the reducing flame, even when treated with tin on charcoal, a colorless glass. The mineral is partially decomposed by hydrochloric acid; more perfectly by strong sulphuric acid, and completely by fusion with carbonate of soda. An imperfect analysis gave the following results:

Silica, - - - - - - - - -	27·90
Zirconia, - - - - - - - - -	66·93
Sesquioxyd of iron with trace of manganese, -	2·57
Water, - - - - - - - - -	2·19
	99·59

These numbers must give very nearly the composition of the material analyzed; but the composition must vary with the degree of alteration, and probably no two specimens would give precisely the same result. The material used was the best I could command, but not so good as might be desired, and I am happy to learn that a more trustworthy analysis will soon be published. On comparing these numbers with Scheerer's analyses of malacone, it appears that the amount of silica is somewhat greater in the original specimens, but the difference is evidently the result of further alteration. The zirconia was examined for other earths and metallic oxyds, but without positive results. No distinct indications of titanium could be obtained with the blowpipe, although it is possible that in the presence of so large an amount of zirconia a small amount might have escaped notice. The zirconia separated from the mineral was converted into chlorid, and the solution when evaporated crystallized to the last drop, forming a large mass of white silky needles. A solution of this chlorid, still acid to litmus paper, turned turmeric paper orange-yellow. In a solution of the same chlorid, oxalic acid produced a voluminous precipitate, which, when the solu-

tion was cold, readily dissolved in an excess of the reagent, and the more readily the larger the quantity of free hydrochloric acid present. If the solution was heated to boiling during the precipitation, the precipitate was not easily redissolved; but by digesting it for several hours with a very large excess of oxalic acid complete solution was finally effected. In a similar solution of the chlorid, a strong solution of sulphate of potash produced a precipitate, which in the presence of free hydrochloric acid readily dissolved in an excess of the reagent if the solution was kept cold. But if the solution was heated during the precipitation, the precipitate did not thus redissolve; but it immediately disappeared on adding a small amount of dilute sulphuric acid. From a solution of the chlorid of zirconium, ammonia throws down a gelatinous precipitate, but this precipitation was entirely prevented by the addition of tartaric acid. The hydrate thus obtained, when heated before the blowpipe, became intensely luminous and changed into a hard granular powder insoluble in all dilute acids. After digesting it, however, for several hours in strong sulphuric acid, it was converted into a sulphate, which dissolved completely on diluting the acid with water. These reactions appear to indicate that the zirconia was quite, if not entirely, free from an admixture of other earths, and the manner in which its reactions are modified by the temperature is the probable explanation of the confusion on the subject which is found in many text-books. The two modifications of zirconia, distinguished by Berzelius, seem to be repeated in many of its compounds, and malacone stands in the same relation to ordinary zircon that the soluble oxalate and sulphate do to the insoluble varieties. The malacone at Rockport is unquestionably in process of alteration, but whether the original mineral was the ordinary zircon or what we may call the normal malacone, I have not yet been able to determine.

ART. XXVII.—*On a possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust;* by JOHN EVANS, F.R.S.*

AT a time when the causes which have led to climatal changes in various parts of the globe are the subject of so much discussion, but little apology is needed for calling the attention of this Society to what possibly may have been one of these causes, though it has apparently hitherto escaped observation.

That great changes of climate have taken place, at all events in the northern hemisphere of the globe, is one of the best estab-

* Proceedings of the Royal Society, xv, p. 46.

lished facts of geology, and that corresponding changes have not been noticed to the same extent in the southern hemisphere may possibly be considered as due, rather to a more limited amount of geological observation, than to an absence of the phenomena indicative of such alterations in climatal conditions having occurred.

The evidence of the extreme refrigeration of this portion of the earth at the Glacial Period is constantly receiving fresh corroboration, and various theories have been proposed which account for this accession of cold in a more or less satisfactory manner.

Variations in the distribution of land and water, changes in the direction of the Gulf Stream, the greater or less eccentricity of the earth's orbit, the passage of the solar system through a cold region in space, fluctuations in the amount of heat radiated by the sun, alternations of heat and cold in the northern and southern hemispheres, as consequent upon the precession of the equinoxes, and even changes in the position of the center of gravity of the earth and consequent displacements of the polar axis, have all been adduced as causes calculated to produce the effects observed; and the reasoning founded on each of these data is no doubt familiar to all.

The possibility of any material change in the axis of rotation of the earth has been so distinctly denied by Laplace* and all succeeding astronomers, that any theory involving such a change, however tempting as affording a solution of certain difficulties, has been rejected by nearly all geologists as untenable.

Sir Henry James,† however, writing to the 'Athenæum' newspaper in 1860, stated that he had long since arrived at the conclusion that there was no possible explanation of some of the geological phenomena testifying to the climate at certain spots having greatly varied at different periods, without the supposition of constant changes in the position of the axis of the earth's rotation. He then, assuming as an admitted fact that the earth is at present a fluid mass with a hardened crust, showed that slaty cleavage, dislocations, and undulations in the various strata are results which might be expected from the crust of the earth having to assume a new external form, if caused to revolve on a new axis, and advanced the theory that the elevation of mountain-chains of larger extent than at present known produced these changes in the position of the poles.

The subject was discussed in further letters from Sir Henry James, the Astronomer Royal, Professors Beete Jukes and Hennessy, and others, but throughout the discussion the principal question at issue seems to have been whether any elevation of a

* Méc. Cél., vol. v, p. 14. † Athenæum, Aug. 25, 1860, &c.

mountain-mass could sensibly affect the position of the axis of rotation of the globe as a whole, and the general verdict was in the negative.

At an earlier period (1848) the late Sir John Lubbock, in a short but conclusive paper in the 'Quarterly Journal of the Geological Society'* pointed out what would have been the effect had the axis of rotation of the earth not originally corresponded with the axis of figure, and also mentioned some considerations which appear to have been absent from Laplace's calculations.

Sir John Lubbock, however, in common with other astronomers, appears to have regarded the earth as consisting of a solid nucleus with a body of water distributed over a portion of its surface; and there can be but little doubt that, on this assumption of the solidity of the earth, the usually received doctrines as to the general persistence of the direction of the poles are almost unassailable.

Directly, however, that we argue from the contrary assumption that the solid portion of the globe consists of a comparatively thin, but to some extent rigid crust with a fluid nucleus of incandescent mineral matter within, and that this crust, from various causes, is liable to changes disturbing its equilibrium, it becomes apparent that such disturbances may lead, if not to a change in the position of the general axis of the globe, yet at all events to a change in the relative positions of the solid crust and the fluid nucleus, and in consequence to a change in the axis of rotation, so far as the former is concerned.

The existence in the center of the globe of a mass of matter fluid by heat, though accepted as a fact by many, if not most geologists, has no doubt been called in question by some, and among them a few of great eminence. The gradual increase of temperature, however, which is found to take place as we descend beneath the surface of the earth, and which has been observed in mines and deep borings all over the world, the existence of hot springs, some of the temperature of boiling water, and the traces of volcanic action, either extinct or still in operation, which occur in all parts of the globe, afford strong arguments in favor of the hypothesis of central heat.

And though we are at present unacquainted with the exact law of the increment of heat at different depths, and though, no doubt, under enormous pressure the temperature of the fusing-point of all substances may be considerably raised, yet the fact of the heat increasing with the depth from the surface seems so well established that it is highly probable that at a certain depth such a degree of heat must be attained as would reduce all min-

* Vol. v, p. 5.

eral matter with which we are acquainted into a state of fusion. When once this point was attained, it seems probable that there would be no very great variation in the temperature of the internal mass; but whether the whole is in one uniform state of fluidity, or whether there is a mass of solid matter in the center of the fluid nucleus, are questions which do not affect the hypothesis about to be considered.

Those who are inclined to regard the earth as a solid or nearly solid mass throughout, consider that many volcanic phenomena may be accounted for on the chemical theory, which has received the support, among others, of Sir Charles Lyell. But apart from the consideration that such chemical action must of necessity be limited in its duration, the existence of local seas of fluid matter, resulting from the heat generated by intense chemical action, would hardly account for the increase of heat at great depths in places remote from volcanic centers; and the rapid transmission of shocks of earthquakes and the enormous amount of upheaval and subsidence as evidenced by the thickness of the sedimentary strata, seem inconsistent either with the general solidity of the globe or any very great thickness of its crust.

The supposition that the gradual oscillations of the surface of the earth, of which we have evidence all over the world as having taken place ever since the formation of the earliest known strata up to the present time, are due to the alternate inflation by gas and the subsequent depletion of certain vast bladdery cavities in the crust of the earth, can hardly be generally accepted.

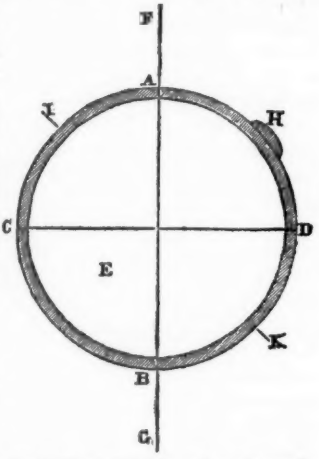
Those who wish to see the arguments for and against the theory of there being a fluid nucleus within the earth's crust, will find them well and fairly stated in Naumann's 'Lehrbuch der Geognosie.'* My object is, not to discuss that question, but to point out what, assuming the theory to be true, would be some of the effects resulting from such a condition of things, more especially as affecting climatal changes. The agreement or disagreement between these hypothetical results and observed facts may ultimately assist in testing the truth of the assumption.

The simplest form in which we can conceive of the relations to each other of a solid crust and a fluid nucleus in rotation together is that of a sphere.

Let A C B D be a hollow sphere composed of solid materials and of perfectly uniform thickness and density, and let it be filled with the fluid matter E, over which the solid shell can freely move, and let the whole be in uniform rotation about an

* 2nd edit., 1858, vol. i, p. 36.

axis FG, the line CD representing the equator. It is evident that in such a case, the hollow sphere being in perfect equilibrium, its axis and that of its fluid contents would perpetually coincide. If, however, the equilibrium of the shell or crust be destroyed, as, for instance, by the addition of a mass of extraneous matter at H, midway between the pole and the equator, not only would the position of the axis of rotation be slightly affected by the alteration in the position of the center of gravity of the now irregular sphere, but the centrifugal force of the excess of matter at H would gradually draw over the shell toward D until, by sliding over the nucleus, it attained its greatest possible distance from the center of revolution by arriving at the equator. The resultant effect would be that though the whole sphere continued to revolve around an axis as nearly as possible in the line F G, yet the position of the pole of the hollow shell would have been changed by 45°, as by the passage of H to the equator the points I and K would have been brought to the poles by spirals constantly decreasing in diameter, while A and B, by spirals constantly increasing, would have at last come to describe circles midway between the poles and the equator.



The axis of rotation of the hollow sphere and that of its fluid contents would now again coincide, and would continue to do so perpetually unless some fresh disturbance in the equilibrium of the shell took place.

If instead of the addition of fresh matter at H we had supposed an excavation or removal of some portion of the shell, a movement in the axis of rotation of the shell would also have ensued, since from the diminished centrifugal force of that portion of the hollow sphere where the excavation had taken place, it would no longer equipoise the corresponding portion on the opposite side at I, and the excavated spot would eventually find its way to the pole.

In order more clearly to exhibit these effects, I have prepared a model in accordance with a suggestion of Mr. Francis Galton, F.R.S., in which a wheel representing a section of a hollow sphere has its axis, upon which it can freely turn, fixed in a frame, which is itself made to revolve in such a manner that the

axis of its rotation passes through one of the diameters of the wheel, and coincides with what would be the axis of the sphere of which the wheel is a section.

In the periphery of the wheel are a number of adjustable screws with heavy heads, so that, by screwing any of them in or out, the addition of matter or its abstraction at any part of the sphere may be represented.

If by adjusting these screws the wheel could be brought into perfect equilibrium, its position upon its own axis would remain unchanged in whatever position it was originally placed, notwithstanding any amount of rotation being given to the frame in which it is hung; but practically it is found that with a certain given position of the screws a certain part of the wheel coincides with the axis of the frame, or becomes the pole around which the sphere revolves. The rim of the wheel is graduated so as to show the position of the poles in all cases, and generally speaking the wheel always settles down after rotation with the pole within three or four degrees of the same spot, if no alteration has been made in the adjustment of the screws, though of course what was the uppermost pole may become the lower one; and in some cases the wheel may be *in æquilibrio* with a projecting screw either above or below the equator, in which case there may be four readings on the circle at the index-point, according as the one pole or the other is uppermost, and the projecting screw is above or below the equator.

With the screws on the wheel evenly balanced, a slight alteration in the adjustment of any of them immediately tells upon the position of what, for convenience sake, may be called the poles, except, indeed, in such cases, as screwing outward those already at the equator, or making similar alterations in the adjustment of two screws at equal distances on either side of one of the poles. If a screw be turned outward so as notably to project at any spot, no matter how near to the pole, it will be found, after the machine has been a short time in revolution, in the region of the equator. Or again, if one or, better still, two opposite screws at the equator be turned inward, they will be found after a short period of revolution at the poles.

Now let us assume for a moment that, though the crust was partially covered by water, the earth, instead of being a spheroid, was a perfect sphere, consisting of a hardened crust of moderate thickness supported on a fluid nucleus over which the crust could travel freely in any direction, but both impressed with the same original rotatory motion, so that without some disturbing cause they would continue to revolve forever upon the same axis, and as if they were one homogeneous body. Let us assume, moreover, that this crust, though in perfect equilibrium on its center of rotation, was not evenly spherical externally,

but had certain projecting portions, such as would be represented in nature by continents and islands rising above the level of the sea.

It is evident that so long as those continents and islands remained unaltered in their condition and extent, the relative position of the crust to the enclosed fluid nucleus would remain unaltered also. But supposing those projecting masses were either further upheaved from some internal cause, or worn down and ground away by the sea or by subaërial agency and deposited elsewhere, it seems impossible but that the same effects must ensue as we see resulting upon the model from the elevation and depression of certain screws, and that the axis of rotation of the crust of the sphere would be changed in consequence of its having assumed a fresh position on its fluid nucleus, though the axis of the whole sphere might have retained its original direction, or have altered from it only in the slightest degree.

An irregular accumulation of ice at one or both of the poles, such as supposed by M. Adhémar, would act in the same manner as an elevation of the land; and even assuming that the whole land had disappeared from above the surface of the sea, yet if by marine currents the shallower parts of the universal ocean were deepened and the deeper parts filled up, there would, owing to the different specific gravity of the transported soil and the displaced water, be a disturbance in the equilibrium of the crust, and a consequent change in the position of its axis of rotation.

Now if all this be true of a sphere, it will also, subject to certain modifications, be true of a spheroid so slightly oblate as our globe.

The main difference in the two cases is, that in a sphere the crust may assume any position upon the nucleus without any alteration in its structure, while in the case of the movement of a spheroidal crust over a similar spheroidal nucleus, every portion of its internal structure must be more or less disturbed as the curvature at each point will be slightly altered.

The extent of the resistance to an alteration of position arising from this cause will depend upon the oblateness of the spheroid and the thickness and rigidity of the crust; while the thicker the latter is, the less also will be the proportionate effect of such elevations, subsidences, and denudations as those with which we are acquainted. The question of friction upon the nucleus is also one that would have to be considered, as the internal matter though fluid might be viscous.

It will of course be borne in mind that the elevations and depressions of the surface of the globe are not, on the theory now under consideration, regarded according to the proportion they bear to the earth's radius, but according to their relation to the thickness of the earth's crust; and that, even assuming Mr. Hop-

kins's extreme estimate to be true, yet elevations or depressions, such as we know to have taken place, of 8,000 or 10,000 feet, bear an appreciable ratio to the 800 or 1,000 miles which he assigns as the thickness of the earth's crust.

It is, however, to be remarked that the extremely ingenious speculations of Mr. Hopkins are based on the phenomena of precession and nutation, and that if once the possibility of a change in the position of the axis of rotation of the earth's crust be admitted, it is not improbable that the value of some of the data upon which the calculations of these movements are founded may be affected.

The supposition of the thickness of the crust being so great seems also not only entirely at variance with observed facts as to the increase of heat on descending beneath the surface of the earth, but to have been felt by Mr. Hopkins himself to offer such obstacles to any communication between the surface of the globe and its interior, that he has had recourse to an hypothesis of large spaces in the crust at no great depth from the surface, and filled with easily-fusible materials, in order to account for volcanic and other phenomena.

But though it may be possible to account for volcanoes upon such an assumption, yet, as already observed, the phenomena of elevation and depression, such as we find to have taken place, and more especially the existence of vast geological faults, cannot without enormous difficulty be reconciled with such a theory.

Taking the increment of heat as 1° Fahrenheit for every 55 or 60 feet* in descent, a temperature of 2400° Fahr. would be reached at about 25 miles, sufficient to keep in fusion such rocks as basalt, greenstone, and porphyry; and such a thickness appears much more consistent with the fluctuations in level, and the internal contortions and fractures of the crust which are everywhere to be observed. Sir William Armstrong, on the assumption of the temperature of subterranean fusion being 3000° Fahr., considers that the thickness of the film which separates us from the fiery ocean beneath would be about 34 miles.

Even assuming a thickness of 50 miles, so as to make still greater allowance for the increased difficulty of fusion under heavy pressure, the thickness of the crust would only form one-eightieth part of the radius of the earth; or if we represent the earth by a globe 13 feet in diameter, the crust would be one inch in thickness, while the difference between the polar and equatorial diameters would be half an inch.

In such a case, the elevation or wearing away of continents such as are at present in existence, rising, as some of them do, nearly a quarter of a mile on an average above the mean sea-

* Page, 'Advanced Text-book of Geology,' p. 30.

level, would cause a great disturbance in the equilibrium of the crust, sufficient to overcome considerable resistance in its attempts to regain a state of equilibrium by a movement over its fluid nucleus.

Whether the thickness of the earth's crust was not in early geological times less than at present, so as to render it more susceptible of alterations in position—whether the spheroid of the fluid mineral nucleus corresponds in form with the spheroid of water which gives the general contour of the globe—whether or no there are elevations and depressions upon the nucleus corresponding to some extent with the configuration of the outer crust, and whether the motion of the crust upon it, besides effecting climatal changes, might not also lead to some elevations and depressions of the land, and produce some of the other phenomena mentioned by Sir Henry James, are questions which I will leave for others to discuss.

My object is simply to call attention to what appears to me the fact, that if, as there seems reason to suppose, our globe consists of a solid crust of no great thickness resting on a fluid nucleus, either with or without a solid central core, and if this crust, as there is abundant evidence to prove, is liable to great disturbances in its equilibrium, then it of necessity follows that changes take place in the position of the crust with regard to the nucleus, and an alteration in the position of the axis of rotation, so far as the surface of the earth is concerned, ensues.

Without in the slightest degree undervaluing other causes which may lead to climatal changes, I think that possibly we may have here a *vera causa* such as would account for extreme variations from a tropical to an arctic temperature at the same spot, in a simpler and more satisfactory manner than any other hypothesis.

The former existence of cold in what are now warm latitudes might, and probably did in part, arise from other causes than a change in the axis of rotation, but no other hypothesis can well account for the existence of traces of an almost tropical vegetation within the arctic circle.

Of the former existence of such a vegetation, the evidence, though strong, is not conclusive. But if the fossil plants of Melville Island, in lat. 75° N.,* which appear to agree generically with those from the English coal-measures, really grew upon the spot where they were now discovered, they seem to afford conclusive evidence of a change in the position of the pole since the period at which they grew, as such vegetation must be considered impossible in so high a latitude.

The corals and Orthoceratites from Griffiths Island and Cornwallis Island, and the Liassic Ammonites from Point Wilkie,

* Lyell, 'Principles of Geology,' 1853, p. 88.

Prince Patrick's Island, tell the same story of the former existence of something like a subtropical climate at places at present well within the arctic circle.

To use the words of the Rev. Samuel Haughton,* in describing the fossils collected by Sir F. L. McClintock, "The discovery of such fossils *in situ*, in 76° N. latitude, is calculated to throw considerable doubt upon the theories of climate, which would account for all past changes of temperature by changes in the relative position of land and water on the earth's surface;" and I think that all geologists will agree with this remark, and feel that if the possibility of a change in the position of the axis of rotation of the crust of the earth were once admitted, it would smooth over many difficulties they now encounter.

That some such change is indeed taking place at the present moment may not unreasonably be inferred from the observations of the Astronomer Royal, who, in his Report to the Board of Visitors for 1861, makes use of the following language, though "only for the sake of embodying his description of the observed facts," as he refers the discrepancies noticed to "some peculiarity of the instrument. . . . . The transit circle and collimators still present those appearances of agreement between themselves and of change with respect to the stars which seem explicable only on one of two suppositions—that the ground itself shifts with respect to the general earth, or that the axis of rotation changes its position."

ART. XXVIII.—*On Fluorescence;* by J. ENEU LOUGHLIN, Philadelphia.

IN the year 1845, Sir J. Herschel published two papers in the 'Philosophical Transactions,' on what he termed the epipolic dispersion of light. His researches were made upon sulphate of quinia and other organic substances, from which researches he deduced the conclusion that the colors came from the surface of the liquid at which the light entered, and that a ray of light having once passed through such a stratum has lost the power of reproducing the same effect. Sir D. Brewster, in 1846, in a paper read before the Royal Society of Edinburgh, drew attention to a similar phenomenon in a solution of the green principle of leaves, and disproved the ideas of Sir J. Herschel, by showing that the light was dispersed not merely at the surface, but for a long distance within the fluid. In 1852, the subject was taken up by Mr. Stokes of Cambridge, and by him ably discussed. He examined many organic substances and arrived at

* Journal of the Royal Dublin Society, vol. i, p. 244.

several valuable conclusions. He was the first to examine the fluorescent nature of glass colored by oxyd of uranium.

In June of the present year, I commenced a series of experiments upon the tinctures and infusions of the leaves, barks, and roots of various plants. A beam of sunlight was the agent employed for illumination. The apparatus, that of Prof. Stokes. Of the specimens examined, those worthy of note, with their results, are given in the following table:

Tinct. of	Quassia root,	light green.
"	Veratrum viride leaves,	greenish-yellow.
"	Aconite root,	orange-yellow.
"	Opium,	greenish-yellow.
"	Belladonna leaves,	yellowish-green.
"	Stramonium leaves,	light green.
"	Nux vomica root,	yellow.

The tinctures were then deprived of color by being filtered through animal charcoal, the object being to remove the coloring matter, as several of them gave reactions which were due to the presence of chlorophyll. On an examination they gave the following results:

Tinct. of	Quassia,	light green.
"	Veratrum viride,	yellowish-green.
"	Aconite,	yellowish-green.
"	Opium,	deep yellow tint.
"	Belladonna,	yellowish-green.
"	Stramonium,	light green.
"	Nux vomica,	light green.

Tinct. of Nux vomica deserves more than passing notice; when deprived of color it gave a faint green tint in place of the original yellow tint. All the alkaloids of the above that could be obtained were examined, viz:

Quassia,	distinct light green.	Codeia,	faint green passing to orange.
Veratria,	yellowish-green.	Atropia,	yellowish green.
Aconitia,	yellowish-green.	Strychnia,	faint green passing to orange.
Morphia,	yellow.		
Narcotina,	yellow.		

The next step was the examination of the fluorescence shown on the pouring of infusions of different substances into water contained in a tall jar, also the fluorescence shown by paper saturated with different infusions. The flame of burning sulphur served as the illuminating agent. The intensity of color varied as is shown in the table.

		Paper.	Liquid.
Infusion of	Esculin,	distinct,	distinct.
"	Cinchona,	pale,	distinct.
"	Quassia,	pale,	distinct.

	Paper.	Liquid.
Infusion of Stramonium,	pale,	distinct.
" Aconite,	indistinct,	pale.
" Veratria,	indistinct,	pale.
" Galls,	distinct,	distinct.

For the light of sulphur was then substituted that of the following illuminating agents, alcohol flame colored by different substances being used, with the accompanying results.

Red by nitrate of strontia,	no action.
Green by nitrate of baryta,	indistinct.
Yellow by chlorid of sodium,	no action.
Violet by chlorid of potassium,	similar to sulphur.
Red by carbonate of lithia,	indistinct.
Blue by blue fire,	very distinct.
Potassium,	distinct.
Magnesium,	no action.

The infusions of the above were spread upon paper and made to receive the solar spectrum. Those that produced elongation of the violet portion were—

Infusion of Cinchona,	very decided.
" Esculin,	very decided.
" Quassia,	very distinct.

Philadelphia, Dec. 8th, 1866.

ART. XXIX.—*Note on Dr. Andrews' paper on the Glacial Drift;** by E. W. HILGARD, Prof. of Chemistry, University of Mississippi.

DR. ANDREWS' interesting paper on the Drift of Illinois, in the January number of this Journal, defines in a precise manner a point which I have heretofore failed to find distinctly elucidated, viz: the exact stratigraphical relations of the great stratified deposits of sand and gravel in the Northwest to such as exhibit unquestionable evidences of *glacier* action, and not only those which, so far as their character was concerned, might equally as well be referred to that of stranded floes, or shore-ice. It seemed strange, indeed, that the former should be absent, where so much of glacial transporting agency was apparent.

The occurrence of glacier-scored rocks is likely, in general, to be confined to a moderate distance from the parent glacier. Apart from the terminal moraine, we find them frozen into the bottom or sides of the glacier, and of course into the corresponding surfaces of the iceberg detached from it. They are therefore liberated by thawing long before those unscored an-

* This Jour., Jan. 1867, p. 75.

gular blocks, which, by falling into crevasses, become enclosed in the mass of the glacier without penetrating to the bottom. It is only when two thick glaciers unite into one at a very acute angle, that scored rocks may become imbedded in the interior of the combined mass, and afterward be conveyed to considerable distances in an iceberg.

Stratification, therefore, must always be the criterion between the moraine and the deposits formed of the ballast dropped by floating ice, though, from the mode of formation, distinct or continuous stratification could not be expected even in the latter.

Dr. Andrews specially mentions the stratification of the drift penetrated in the Chicago tunnel, and that by great care it can be observed in the greater part of the formation. The latter is therefore clearly not of the moraine character—it is the result of the combined action of water and floating ice, as required by the hypothesis advanced in the paper referred to by him. Nor does the occurrence of the true "glacial drift" beneath the stratified beds in any way detract from the importance of the latter, and the necessity of assigning the origin of their "thick masses" to a cause more powerful in degree and more widely active, than is implied in the sentence quoted from Dana's Manual.

While angular blocks do occur in the stratified drift of middle Illinois and Missouri, they, nevertheless, manifestly decrease in proportion to those of rounded form, as we advance southward; and the lithological composition of the "Orange Sand" of the Southwest is precisely such as we should expect to result from the modification, in proportion to distance and lower latitude, of the agencies to which the stratified drift of the Northwest seems to owe its conformation.

University of Mississippi, Feb. 2, 1867.

ART. XXX.—*On Naphtha and Illuminating Oil from Heavy California Tar (Maltha);* by B. SILLIMAN.

HAVING lately had an opportunity to examine a specimen of "surface oil," so called, from Santa Barbara county, in California, I present the following experimental results in the hope that they may not be without interest, as an addition to our knowledge of one extreme of that class of hydrocarbons which occur in nature in the fluid form, and of every density, from those which are but little lighter than water down to the lightest naphtha found in a natural state.

It is proper to state that the chemical examination of this sample had chiefly a technical object, to prove whether or not illuminating oil of good quality could be obtained from the dis-

tillation of so dense a body. The experiments were conducted on quantities of from five to ten gallons each. The crude oil was very dark, almost black, transmitting yellow brown light in thin films. At ordinary temperatures (60° F.) it is a thick, viscid liquid, resembling coal tar, but with only a very slight odor.

Its density at 60° F. is 0·980 or 13° Baumé. It retains, mechanically entangled, a considerable quantity of water, which is neutral in its reaction. The odor of sulphydric acid, which is very decided in this product, as I have noted in its locality, had entirely disappeared in the specimen under consideration.

The tar froths at the commencement of distillation, from escape of watery vapor. It yields by a primary distillation no product having a less density than 0·844, or 37° B. at 52° F.

Distillation to dryness produced in two trials an average result as follows:

Oil having a density of ·890 to 0·900, - -	69·82
Coke, water, and loss, - - - - - -	30·18
	100·00

In one of these trials the product was divided as follows:

Oil, of density 29° B. at 52° (·885 sp. gr.), -	50·0
" " 24·75 " 53° (·908 "), -	17·5
Coke, water, loss, &c., - - - - -	32·5
	100·0

The coke is very large in quantity, strong, and is a good fuel, resembling gas-house coke. The odor of ammonia is given off toward the close of the distillation.

It is well known to distillers of petroleum that by the process called "cracking," heavy oils unfit for illumination are broken up into bodies of less density, from light naphtha to the heavier illuminating and lubricating oils. This process is simply the application of a carefully regulated heat producing a slow distillation. By this treatment the molecules apparently rearrange themselves into groups of different density, which by a subsequent distillation are divided into fractions (or "heaps" as Mr. Warren calls them) of tolerably constant boiling points.

The first distillate, having a density of about ·890 at 60° F., treated in this manner, yielded a product having a density of about ·885 at 60°, or only 1° Baumé lower than before distillation. After treatment with sulphuric acid and soda and redistilling from soda, it had a density of ·880 at 60° F. Upon redistilling, 100 measures of this last distillate yielded—

Light oil having a density of about ·834 at 60° F.,	21·58
Heavy " " " " ·880 " 66° F.,	37·41
" " " " " ·916 " 64° F.,	34·53
Coke, &c., - - - - - - - - -	6·48
	100·00

In another experiment undertaken with a view to "cracking," &c., treating and redistilling with soda, the products were as follows, stated in percentages of the whole quantity operated on, the several steps being as before.

Naphtha,*	sp. gr.	about	·760	at	60°	F.,	-	-	11·33
Oil,†	"	"	·836	"	"	-	-	66·22
"	"	"	·893	"	"	-	-	12·67
"	"	"	·921,		-	-	-	3·56
Loss, -	-	-	-	-	-	-	-	-	6·22
									100·00

The illuminating oil from both these experiments, after treatment with sulphuric acid and soda in the usual manner, acquired an agreeable odor, a light straw-yellow color, and burned as well in a lamp as good commercial oil.

With a view to test the effect of heat aided by pressure in breaking up the heavy hydrocarbons—a method of treating heavy hydrocarbon oils patented in 1866 by Mr. James Young of Glasgow—a portion of the first distillate from the crude oil was subjected during distillation to a pressure of ten to fifteen pounds to the square inch, in an apparatus adapted to the purpose, the distillate thus obtained being about the same density as in the first named experiment, ·890 at 60° F.

From this distillate were obtained, after the ordinary treatment with sulphuric acid and soda, the following products:

Light oil,	sp. gr.	·825	at	60°	F.,	-	-	19·2 p. c.
Heavy "	"	·885	"	"		-	-	25·86 "
" "	"	·918	"	"		-	-	38·14 "
Coke, loss, &c.,	-	-	-	-	-	-		16·80 "
								100·00

The illuminating oil from the last experiment flashed at 80° F. and lighted on the surface at 85° F., showing the presence of naphtha or some very light body, the quantity of which cannot be very considerable. The light oil could with care be taken off in practice without materially diminishing the yield of illuminating oil. It would be rash to conclude that there may not be an important economical advantage in employing in the large way Mr. Young's method of treatment under pressure, over that of "cracking" by a regulated heat alone. It is highly probable that there would be found an important saving of time, as under a regulated pressure and a corresponding increase of temperature, the transformation of the heavy oils into a mixture of those of less density, will occur more speedily. The experiments herein mentioned gave nearly the same result whether

* This naphtha caught fire from a match at an atmospheric temperature of 56° F.
† This oil flashed at 113° F. and ignited at 124° F.

pressure was used or not; a certain loss, all falling upon the lighter portions, was found to result from leakage of the apparatus under pressure, which in the larger way of operating commercially could be avoided.

No paraffine could be detected by refrigerating the heavy oils obtained in these distillations in a mixture of salt and ice. It is no doubt the absence of this body from the series of products obtained from the California oils generally, that accounts for the illuminating oil burning well at a density considerably below the commercial standard for oil obtained from Pennsylvania petroleum—a difference enhanced also by the absence of any considerable quantity of light naphtha. The lubricating oils of this series, likewise free from paraffine, retain on this account their fluidity at low temperatures.

The light oils obtained in this series of experiments correspond respectively to 12·96, 14·56 and 18·96 per centum of the crude oil. The total commercial products are about 60 per cent of the crude body, which likewise yields sufficient coke to supply the fuel required in the distillations.

In the large way, by returning the lightest oils to the heavier portions in the successive distillations and employing Mr. Young's method by pressure, it is probable the product of light or illuminating oils may be raised in these very heavy natural products to 30 per cent.

It is evident from these experiments that heavy hydrocarbon oils containing no naphtha are convertible into oils of the naphtha series under the action of heat by molecular transformations, the excess of carbon being left behind as coke; each successive distillation eliminating a new but always a diminished portion of carbon.

I am indebted to Mr. A. J. Corning, formerly assistant to Mr. Warren, for conducting this research under my direction; and to Messrs. Downer and Merrill, of the kerosene works in South Boston, I am under many obligations for the permission to employ their operative laboratory in conducting this research. As before stated, the research had chiefly a technical object, the points of scientific interest being subordinate in a great degree. For the crude material operated on I am indebted to the California Petroleum Company, from whose estate it was derived.

Note.—In the number of this Journal for May, 1865, I published the analyses of a sample of crude petroleum, believed by me to be from a well-known spring, called the "Pico Spring," in Santa Barbara county, California. This sample, as well as a subsequent one in larger quantity, came to me under seal from responsible parties and no question of its authenticity, until a very recent period, ever reached me. It is now confidently asserted,

by certain parties, that the samples in question were sophisticated by the addition of refined commercial petroleum. An inquiry instituted privately by me has elicited from the parties immediately concerned in its transmission only an emphatic denial of the charge of falsification, and if any such fraud has been perpetrated I am well persuaded that the responsibility falls elsewhere. My investigations in this direction are unrelaxed, and the truth cannot remain long concealed.

New Haven, January 14th, 1867.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the influence of the absorption of heat upon the formation of dew.*—The long pending controversy between Magnus and Tyndall in relation to the absorptive power of aqueous vapor, appears to have been brought to a conclusion by an experiment of Magnus based upon the principle of the equality of the radiating and absorbing powers of bodies. The apparatus employed consisted of a horizontal brass tube, which could be heated red hot by means of a row of burners, and served to conduct the gases and vapors to be examined. One end of this tube was turned up vertically so as to form the radiating jet of gas or vapor; the other was connected with a bellows, while by means of interposed branched tubes and other apparatus the air driven through could be previously dried or passed through water at a known temperature: finally, pure aqueous vapor could be employed to form a jet without admixture of air. A thermometer was suspended over the jet so that its temperature could be determined with sufficient accuracy. The heat radiated from the jet of gas or vapor was allowed to fall upon one surface of a thermo-pile provided with conical reflectors and placed within a double chest of card-board. The effect produced by the radiation of heat from other sources than the jet of gas or vapor was compensated by placing a second and precisely equal source of heat upon the other side of the thermo-pile. With this arrangement a jet of dry air produced by radiation a deviation of the galvanometer which amounted to only three divisions of the scale. When the air was passed through water the deviation remained almost the same. When dry carbonic acid gas was passed through the hot brass tube the deviation amounted to 100–120 divisions, and common illuminating gas gave nearly the same deviation. When air was passed through water heated to 60°–80° C. the deviation rose to 20, but very gradually, while the deviations produced by carbonic acid and illuminating gas were sudden, and rapidly rose to their maxima. When the water boiled so as to produce clouds in the jet of air the deviation exceeded 100 scale divisions, and the same result took place when no jet of air was forced through but the steam alone formed vesicular vapor. When no vesicular vapor was present the galvanometer gave no greater deviation than 20, no matter how much steam might be present.

The greater deviation invariably accompanied the formation of vesicular vapor. From these experiments Magnus concludes that transparent or proper aqueous vapor has a radiating and absorbing power but little greater than that of air, and consequently that the absorptive power of air which contains transparent vapor differs but little from that of dry air. In conclusion the author brings forward an argument based upon the formation of dew. If aqueous vapor were as good an absorbent of heat as Tyndall supposes, dew could never be formed at all, since the vapor necessary for its formation would form a covering over the surface of the earth and prevent radiation. In the tropics, where the atmosphere is loaded with moisture, the dew is very heavy. If the vapor of water possessed as high an absorptive power as Tyndall attributes to it, only a small portion of the heat radiated from the earth could reach the clouds, and the effect of clouds in preventing the formation of dew could not be explained. The conclusions of Frankland in regard to the ice period, and of Tyndall for certain climatic phenomena, based upon the absorptive power of aqueous vapor, remain unchanged if we substitute vesicular vapor. The author further cites the experiments of Cooke and Secchi in regard to the absorption of light by aqueous vapor as shown by the spectroscope, a few dark lines being produced which scarcely diminish the total intensity of the light.—*Pogg. Ann.*, cxxvii, 613.

W. G.

2. *On some new forms of electrical apparatus.*—Töpler and Holtz have described independently, but at about the same time, new forms of electrical machines which may be regarded most simply as rotating electrophori. Töpler's machine, which is the simpler of the two, consists essentially of a circular plate or disc of thin vulcanized rubber, gutta percha or glass, mounted upon a vertical axis and caused to rotate rapidly by means of a band and wheel. The disc is coated upon each side with two segments of tin foil, a free space being left between the segments, while the coatings are connected over the edge of the disc by strips of foil. A piece of hard rubber forming a segment of a circle is then excited by friction and placed near and parallel to the lower coated surfaces of the revolving disc. This lower surface becomes electrical by induction, the opposite electricity being driven over the edge to the upper surface of the plate. As the plate revolves, one under segment of tin foil is removed from the inductive action of the excited surface, and the second becomes parallel to it, when the free electricity is decomposed as before. Two isolated conductors are placed above and parallel to the disc, and each carries at one end a light spring or strip of tin foil which rests upon the upper surface of the disc. The two strips are so arranged that as the disc revolves one strip is just leaving a segment of tin foil as the other is brought into contact with it. In this manner the electricity driven to the upper surface is first carried off by one conductor, while the electricity retained upon the lower surface at first, as the plate revolves, passes to the upper surface and is drawn off by the second conductor. The same process then takes place with the second coating, and so on alternately. It will be seen that so far the apparatus is exactly equivalent to an electrophorus, and that the action, though powerful at first, must diminish rapidly as the inductor loses electricity. To remedy this

difficulty a second but smaller disc of glass is placed on the same axis, coated with tin foil in the same manner and provided with a similar inductor and similar conductors. This second inductor is connected with one pole or conductor of the upper and larger plate. Of the two similar conductors belonging to the lower plate, one is connected with the earth while the other is connected with the inductor of the upper plate. In this manner, as the discs rotate, the earth furnishes a constant supply of electricity, and the action of the machine is remarkably powerful. Holtz's machine depends upon similar principles, but could hardly be made intelligible without figures. Several of them have already arrived in this country. A simpler form of apparatus of the same kind has since been described by Bertzch. All these instruments appear to be very much more powerful than plate electrical machines of the same size, and they have the advantage of working with but a small application of force.—*Pogg. Ann.*, cxxv, 469; also Holtz in *Pogg. Ann.*, cxxvii, 320; Bertzch in *Cosmos*, No. 7, 1866. W. G.

3. *On nitrites of cobalt and nickel.*—O. L. Erdmann has described a series of double nitrites which, while not wanting in interest from a theoretical point of view, are of some importance in analytical chemistry. Nickel gives, with potash and nitrous acid, a soluble double salt, crystallizing in brownish-red octahedrons and having the formula $2KO,NO_3+NiO.NO_3$, as already found by Lang and Rammelsberg. When a solution of this salt is mixed with one of chlorid of calcium, or when nitrite of potash is added to a solution containing both nickel and calcium, a yellow crystalline precipitate is formed which is very slightly soluble in cold but much more soluble in hot water. The solution is green, but the salt is always more or less decomposed. When the salt is formed slowly it crystallizes in well-defined regular octahedrons. Its constitution is represented by the formula

$$CaO,NO_3+NiO.NO_3+KO,NO_3 \text{ or } \left.\begin{matrix}NiO\\CaO\\KO\end{matrix}\right\}3NO_3.$$

The corresponding barium salt has been described by Lang. It is also very slightly soluble in water, and crystallizes in microscopic cubes with octahedral faces. The strontium salt forms reddish-yellow crusts consisting of microscopic cubes. Cobalt forms precisely analogous compounds. The triple nitrite of cobalt, potash and lime, forms a black-green crystalline precipitate. The salts of cobalt with barium and strontium, have a still more beautiful green color, but like the calcium salt are decomposed by washing. The ammonia nitrite of nickel, which Erdmann terms nitrite of diamin-nickel, forms small cherry-red brilliant crystals having the formula $2NH_3.NiO,NO_3$, and easily decomposed. Fischer's nitrite of cobalt and potash, which according to Stromeyer has the formula $Co_2O_3.2NO_3+3KO,NO_3$, is, according to Erdmann, a mixture of two different salts, one of which is formed in an acid and the other in a neutral solution. The salt formed in a neutral solution has the formula $\left.\begin{matrix}3CoO\\3KO\end{matrix}\right\}6NO_3+HO$. It is a yellow crystalline powder composed of microscopic cubes. The author has satisfied himself that no absorption of oxygen takes place during the formation of this salt,

and consequently that the cobalt is present as protoxyd, and not as sesquioxyd as Stromeyer supposed. The salt is insoluble in cold water, but dissolves in boiling water with a red color. When a solution of chlorid of cobalt is acidulated with acetic acid and then a solution of nitrite of potash added, a yellow precipitate of a brighter color than that of the last mentioned salt is formed. If the quantity of acetic acid is small the solution gradually becomes less acid and finally weakly alkaline. The first mentioned salt is then deposited, so that in analyses the substance weighed is usually a mixture of the two salts. The salt precipitated from an acid solution has the formula $\left.\begin{matrix}2CoO\\3KO\end{matrix}\right\}6NO_3+3HO$, which may perhaps be written more correctly $\left.\begin{matrix}2CoO\\3KO\\HO\end{matrix}\right\}6NO_3+2$ aq. The corresponding ammonia salt has the formula $\left.\begin{matrix}2CoO\\3NH_4O\\HO\end{matrix}\right\}6NO_3+2aq$, and consists of microscopic cubes slightly soluble in cold water with a yellow color. When a solution of chlorid of cobalt is heated with much sal-ammoniac, and then with a solution of nitrite of potash, a yellow precipitate is formed in brilliant micaceous scales, while the liquid becomes strongly acid and evolves nitrous acid. The mother liquor afterward deposits brown crystals. The yellow salt appears not to have a constant composition, but the brown compound is easily purified by recrystallization; it has the formula $2NH_3.Co_2O_3, 3NO_3+KO, NO_3$. With nitrate of silver this salt gives a yellow or orange precipitate which has the formula, $2NH_3.Co_2O_3, 3NO_3+AgO, NO_3$. The author describes a corresponding ammonium salt isomorphous with the potassium compound. When a solution of chlorid of cobalt is heated with an excess of a mixture of ammonia and nitrite of potash, the solution absorbs oxygen on standing and becomes darker, depositing finally bright brownish-yellow leaf-like crystals which are with difficulty soluble in cold water but dissolve pretty easily in hot water, and crystallize from the solution in the form of brilliant deep yellow flat needles or leaves. This compound has the formula $3NH_3.Co_2O_3, 3NO_3$, and appears to be the nitrite of the base $3NH_3.Co_2O_3$, the sulphite of which was described by Künzel.—*Journal für prakt. Chemie*, xcvii, 385. W. G.

4. *On the synthesis of alcohols by means of chlorinated ethers.*—Lieben has studied the action of zinc-ethyl upon the chlorinated ethers, and has obtained results of great interest closely connected with those of Frankland on the structure of the organic acids. By the action of zinc-ethyl upon bichlor-ether, $\left.\begin{matrix}\epsilon_2H_3Cl_2\\\epsilon_2H_5\end{matrix}\right\}\Theta$, Lieben and Bauer had already obtained ethylo-chlor-ether, $\epsilon_2H_3.Cl.\left.\begin{matrix}\epsilon_2H_5\\\epsilon_2H_5\end{matrix}\right\}\Theta$. By the action of concentrated iodhydric acid upon this body the chlorine is in a great measure replaced by hydrogen, iodine is set free and a heavy oil is formed containing iodid of ethyl, ethylated chlor-ethyl, and ethylated iodid of ethyl. Ethylated iodid of ethyl has the constitution and boiling point of Wurtz's iodid of butyl. With acetate of silver it yields ethylated

acetate of ethyl and butylene. Ethylated acetate of ethyl, by boiling with concentrated caustic potash yields ethylated alcohol, which has the properties of the butyl-alcohol obtained by Würtz from the fusel oil of beet sugar. Lieben thinks it probable, however, that this body is only isomeric with butyl-alcohol and is identical with butylene-hydrate. By the action of iodhydric acid upon biethylated ether, $\left.\begin{matrix}\mathrm{C_2H_3}\cdot\mathrm{C_2H_5}\cdot\mathrm{C_2H_5}\\ \mathrm{C_2H_5}\end{matrix}\right\}\mathrm{O}$, Lieben has obtained biethylated iodid of ethyl, from which it is probable that biethylated ethyl-alcohol will be obtained. This would be a secondary or tertiary alcohol isomeric with normal butyl alcohol. The author promises a continuation of these researches which can hardly fail to result in the formation of a great number of new alcohols with their corresponding acids.—*Sitzungsber. der Wiener Acad.*, liv, 225, quoted in *Chem. Centralblatt*, No. 1, 1867. W. G.

5. *Graphitoidal Boron, a compound of aluminum.*—St. Claire Deville communicated to the French Academy at its session on the 21st of January the fact that in connection with Wöhler he had recently ascertained that the variety of boron called graphitoidal, originally described by them, was not pure boron, but a definite compound of boron with aluminum. It is formed in the preparation of crystallized boron by means of aluminum, especially when the temperature is not sufficiently high. This aluminic borid crystallizes in very fine hexagonal plates, of the color of pale copper, is perfectly opaque and possesses a metallic luster. According to W. H. Miller these plates belong to the monoclinic system. Heated in the air, it does not burn, but is blued like steel. In chlorine it burns forming aluminic and boric chlorids. It is slowly soluble in hot concentrated chlorhydric acid, and in a hot solution of sodic hydrate, evolving hydrogen. In very strong nitric acid it dissolves easily. The addition of ammonic carbonate to this solution precipitates a basic aluminic borate. In the analysis the boric acid was expelled from this precipitate by fluohydric acid, and then the fluorine by sulphuric acid. Two analyses gave 54·02 and 54·91 per cent respectively of aluminum, corresponding to the formula $\mathrm{AlB_2}$.—*L'Institute*, 1867, p. 27.

6. *Tests for glucose.*—Braun uses a solution of picric acid in 250 parts of water. The glucose solution, containing a little caustic soda, is heated to 90°, a few drops of the picric solution are added, and the whole raised to ebullition; the presence of glucose is indicated by a blood-red coloration due to the production of picramic acid. Cane sugar does not produce this change.—*J. pr. Ch.*, xcvi, 411.

Franqui and deVyvere modify Böttger's test by using an alkaline solution of the bismuthic oxyd. It is prepared by precipitating bismuthic nitrate by a large excess of potash, and adding gradually to the mixture, moderately heated, a solution of tartaric acid; the precipitate dissolves completely, even before the liquid becomes neutral. If a few drops of this reagent be added to diabetic urine, and the whole be boiled, the liquid speedily becomes black and throws down a deposit of metallic bismuth.—*Jour. Med. Bruz.*, 1865, 359.

7. *Hydrocarbons from animal fats.*—Warren and Storer have examined the products resulting from the destructive distillation of a lime soap, produced by the saponification of "menhaden oil," from the *Alosa*

menhaden. The crude hydrocarbons thus obtained were distilled in a current of steam, the distillate treated with sulphuric acid and soda, and again distilled. The rectified product "so closely resembled refined coal oil and petroleum in odor, color, and illuminating properties, that it could hardly be distinguished from these." Their special investigation, however, was confined to the naphtha obtained from the crude product by Warren's process of fractional condensation; indicating by this name, all those hydrocarbons which passed the hot condenser at 220° C. By repeated distillations of this sort, occupying nearly a year, sixteen bodies of constant boiling point were obtained, belonging to several series. To the hydrid series (C_nH_{2n+2}) the hydrids of amyl, caproyl, œnanthyl and capryl; to ethylene series (C_nH_{2n}) amylene, caproylene, œnanthylene, caprylene, pelargonene, rutylene, margarylene, laurylene; and to the benzole series (C_nH_{2n-6}) benzole, toluole, xylole, isocumole. 23·5 per cent consisted of isocumole and rutylene; 13·3 per cent of xylole; 12·5 per cent of caprylene and hydrid of capryl; and 10·2 per cent of margarylene. The importance of this research in relation to the geological production of petroleum is at once evident.—*Mem. Am. Acad.*, new series, ix, 177.

8. *Naphtha from Rangoon petroleum.*—Warren and Storer communicate also the result of the examination, by fractional condensation, of the naphtha from Rangoon petroleum. Owing to the limited supply of material in their hands, they regard their results as only partially complete. Of the hydrid series they assume the presence in this naphtha of the hydrids of œnanthyl, capryl, and pelargonyl; of the ethylene series, they obtained rutylene, margarylene, laurylene, cocinylene, and probably pelargonene; and of the benzole series they regard the presence of xylole and isocumole as probable.—*Mem. Am. Acad.*, new series, ix, 208.

9. *Synthesis of petroleum.*—Berthelot has recently raised the question whether the hydrocarbons found so abundantly in many places may not be of inorganic origin. How otherwise can their presence in volcanic vapors and even in meteorites be accounted for? Without presuming to decide the matter, he thinks it of interest to show, from his recent researches, how such compounds could be formed from purely inorganic materials. If we admit that the alkaline metals may exist in the earth's interior, a hypothesis by no means improbable, the production of these hydrocarbons necessarily follows. For by the action of these alkaline metals upon carbonates, Berthelot has shown that acetylids result; as for example from calcic carbonate, is produced calcic acetylid (C_2Ca). By the action of water upon these compounds free acetylene is evolved; thus $C_2Ca+(H_2O)_2=C_2H_2+CaH_2O_2$: but under the conditions of pressure and temperature there existing, the acetylene would at once be condensed, forming the benzole series; $(C_2H_2)_3=C_6H_6$ or benzole. But from the action of steam upon the alkaline metals, hydrogen would be evolved; and this hydrogen, being condensed with the acetylene, would produce the ethylene series ($C_2H_2+H_2=C_2H_4$); or even the hydrid series ($C_2H_2+H_4=C_2H_6$ ethylic hydrid), all of which series occur in natural hydrocarbons. "We can thus conceive the production by a purely mineral method of all the natural hydrocarbons. The intervention of heat, water, and alkaline metals,—lastly the tendency of the carbides to unite together so as to form matters more condensed, suffice

to account for the formation of these curious compounds. This formation can also be effected in a continuous manner since the reactions which produce it are incessantly renewed."—*Ann. Ch. Phys.*, December, 1866.

10. *Silvering upon glass.*—Having occasion recently to silver some thin microscopic glass, several processes were tried with indifferent success, until finally I hit upon Bothe's method as modified by Böttger (J. pr. Ch., xcii, 494) which afforded most excellent results. Its simplicity, economy, and satisfactory performance induce me to reproduce it here.

7·8 grams of argentic nitrate are dissolved in 60 cc. of water and the solution is divided into two equal portions. A solution of 3·11 grams potassio-sodic tartrate (Rochelle salt) in 1420 cc. of water being brought to *active ebullition*, one of the above portions of the silver solution is gradually added, the boiling is continued 8 or 10 minutes, the whole is allowed to cool and is then filtered. This is the reducing solution.

To the second portion of the silver solution, caustic ammonia is added till the precipitate is *almost* redissolved, care being taken to avoid an excess, and then 355 cc. of water being added, the whole is filtered.

To silver the glass, equal portions of these two fluids, thoroughly mixed and perfectly clear, are poured upon it. After the lapse of about *ten minutes*, a most brilliant layer of metallic silver is deposited, which may be thickened to any desired extent by repeating the process. The film is protected by a layer of varnish. G. F. B.

11. *Nodal figures in organ pipes;* by Dr. August Kundt.—In continuing his acoustic researches (see this Journal, Sept., 1866,) Dr. Kundt has made some farther very interesting discoveries, which are of great theoretical importance and very useful in the lecture room. First he substitutes fine ground or precipitated silicic acid in the place of lycopodium: the silica can be freed from all moisture by ignition, is not acted upon by gases, is equally light, etc. Repeating the experiment reported in the September number of this Journal, he discovered that the *silica forms very thin vertical partitions often across the whole section of the tube while vibrating*, dropping down at the close of the motion, thus forming the transverse ripplings there spoken of.

Taking now an organ pipe—say the mouth-piece of a common tin whistle to which a clean dry glass tube is attached by means of a rubber tube—and intonating any of the higher tones of the pipe, and, if necessary, aiding the silica evenly distributed in the horizontal tube by a few light taps to follow the undulations of the air,—we see a nodal figure of great beauty and distinctness appear, most readily in a closed, but also in an open, pipe. The whole pipe is divided into oval nodal spaces, distant half a wave-length from one another, and between them we see fine, parallel and very distinct, nearly equidistant transverse ripplings, resulting from the transverse walls raised in the tube during the vibration. The experiment is so easily repeated (blowing by means of a bellows or even the mouth) and the nodal figures so exceeding beautiful both during and after the intonation, and the same may so easily be preserved thereafter, that every reader ought to repeat the experiment. Measuring the distance of any two nodal areas gives with great accuracy the half-wave-length of the tone; the same pipe may in quick succession—after

merely shaking up the silica in the pipe—be made to give different tones, the pitch of which is easily ascertained by the ear, and the length of whose waves is found to coincide admirably with the calculation based upon the first determination and the interval—or the velocity of sound and the number of vibrations. The known methods of Hopkins, König, and others, for representing the vibrations of the air in organ pipes are very deficient: Kundt's method is as complete as it is simple.

But it shows more than anticipated. If repeated by means of a vertical pipe, with a *gas-flame* in place of the silica, this flame shows a corresponding stratification in dark and bright layers—the luminous strata evidently due to an accumulation of carbon particles. The mutual distance of these luminous strata is nearly proportional to the length of the tube. From this and other facts Dr. Kundt concludes that the rippling of the silica and the stratification of the flame probably are due to the higher or *secondary tones* (obertöne of Helmholtz) coëxisting with the one produced, and whose wave-length is measured by twice the distance between two consecutive nodal areas.

For farther detail, and many other interesting results, we must refer to the paper of Kundt in Poggendorff's Annalen, 1866, cxxviii, 337–355 and 496. In the same volume, page 610–613, the same investigator gives an interesting method for the observation of the vibratory forms of plates by means of reflection. G. H.

12. *Influence of the interior friction of the air on the transmission of sound;* Stefan (Acad. Vienna, April, 1866).—The analytical results are:

(1.) The velocity is greater for the tones of higher pitch; for the highest tones it only is one-thousandth of a millimeter [so that dispersion of sound is practically impossible?].

(2.) The amplitudes of plane progressive waves decrease in a geometrical progression, the ratio of which is proportional to the square of the number of vibrations. The amplitude is reduced to one-ninth in 1000 meters for 10,000 vibrations, or in 100 meters for 30,000 vibrations per second.

(3.) In stable vibrations the wave-length cannot be greater than four times the mean excursion of a molecule of the air (as assumed in Krönig's theory of the construction of gases).

(4.) The amplitudes in stable waves decrease in geometrical progression of the time, the ratio being proportional to the square of the number of vibrations. Thus the amplitudes of tones of 1000, 10,000, and 30,000 vibrations will in 100, 1, and $\frac{1}{10}$ second be reduced to one-half their original amount.—*L'Institut*, 1866, p. 271. G. H.

13. *Interferential tones.*—In May, 1866, Stefan communicated to the Academy of Vienna some interesting experiments, giving for tones, the vibratory motion of which cannot be contested, a transformation perfectly analogous to the theoretical decomposition of light-waves traversing a quartz crystal in the direction of the axes and producing the rotation of the plane of polarization.

A square plate exhibiting two nodal lines at right angles will produce a more intense tone if the two opposite sectors (having the *same* motion) are covered with a card of their own figure and size: for then the parts

of the plate adjacent to these will alone move the air with concordant impulses. If now the card (or the plate) be rotated n' times a second, the intensity will show n' periods per second; if the amplitude of the original vibration of the plate be a, the amplitude corresponding to the rotation of the card be a', and the pitch of the tone of the plate n, we have $a=a'\sin 2n'\pi t$, and hence the original tone $A=a\sin 2n\pi t$ becomes

$$A=a'\sin 2n'\pi t\sin 2n\pi t=\frac{a'}{2}\cos 2(n-n')\pi t-\frac{a}{2}\cos 2(n+n')\pi t,$$

that is *two tones of pitch $n-n'$ and $n+n'$* (or a *graver* and a *higher* tone) *will be heard instead of the primary tone of pitch n.*

Thus Stefan found that a plate giving the tone *fis*$_2$, which according to Quincke's table (this Journal, Nov. 1866, p. 417) corresponds to $185=n$ vibrations, produces, when $n'=10$ rotations per second, the tones $f_2=174{\cdot}6$ and $g_2=196$, which are very nearly 185 ± 10. L'Institut gives *fis*$_1$, etc., no doubt by a mistake in translating Stefan's German notation.—*L'Institut*, 1866, No. 1710, p. 327; *Cosmos*, 1866, vol. iv, pp. 458, 459. G. H.

14. *Foucault's silvered objectives for observations of the sun.*—A very ingenious method for close observation of the solar disc was communicated by Foucault in September, 1866. Having noticed that no heat and very little light is transmitted through the thin bright silvering of his glasses, he coated the outer surface of the objective of a refracting telescope with such silvering, and found, as he expected, that all heat-rays were reflected, as also the greater part of the light, so as to permit only a pale bluish-violet to pass through. LeVerrier reported, October, 1866, most favorably as to the results obtained by a 9-inch refractor (equatorial). No heat could be felt in the very focus of the objective directed toward the sun—thus freeing all solar observations of a very great cause of error. Furthermore only the ultra-red rays are really absorbed; all others are, as the prismatic spectrum shows, only diminished in intensity so as to give a steady (calme) and pure image of the sun, showing all detail of outline and color with excellent definition, and permitting a magnifying power of 300.—*L'Institut*, 1866, pp. 281, 313; *Cosmos*, 1866, iv, 387, 430. G. H.

15. *Lead-thallium glass* has greater density and refracting power than common lead-flint glass; 300 pure sand, 200 minium, and 335 carbonate of thallium (instead of the usual 100 carbonate of potassa), give a glass of density 4·235, index of refraction 1·71, and only very slight yellowish tint. It has been made in England.—*L'Institut*, 1866, p. 320. G. H.

16. *Expansion of water and mercury;* A. Matthiessen.—The results of this very elaborate investigation are—the volume of water at any temperature $t°$ C. is $V=a-bt+ct^2-dt^3$, the volume at 4° C. being 1 and the coëfficients

	for $4°<t<32$	for $32<t°<100°$
a	1·000138845328	0·999695
b	0·00000579896	0·000000
c	0·000000752824	0·0000054724
d	0·000000007173	0·000000011260

The coëfficient of expansion for a volume of mercury he finds from five series of determinations (per degree C.)

0·0001812

Regnault found 0·0001815.

—*Poggendorff's Annalen*, 1866, cxxviii, 512–540. G. H.

17. *On the expansion of crystals.*—Fizeau has invented a method for the exact determination of the coëfficient of expansion of small solids (only a few millimeters thick), and obtained important results thereby (Cosmos, 1865, xxvi, 641). The following is the substance of a later and more elaborate research of Fizeau.

Let the coëfficient of expansion in the direction of the three axes of elasticity of any crystal be represented by α, α', α'', then the expansion D in any direction determined by the three angles δ, δ', δ'', will be

$$D = \alpha \cos^2 \delta + \alpha' \cos^2 \delta' + \alpha'' \cos^2 \delta'',$$

and the cubical expansion is

$$c = \alpha + \alpha' + \alpha'',$$

which is the same as D for $\delta = \delta' = \delta'' = 54° 41'$, or in a direction at right angles to the surface of a regular octahedron whose axes coincide with the directions of α, α', α''.

Fizeau determines α for three temperatures (viz., 20°, 40° and 60° C.) from which he deduces the value for α at any temperature ϑ, and also the variation of α for each degree, that is $\frac{\Delta\alpha}{\Delta\vartheta}$, which is a constant quantity.

He puts the crystal with one side on a platinum support, and places a polished glass plate above the upper surface so as to leave a small interval; by means of a telescope he counts the number of fringes passing beyond certain fixed points of the support, these Newtonian rings being produced by an alcohol flame with salt, and changed by heating the whole apparatus. For the detail of this method we must refer to the original paper—we can here only give a tabular view of the results obtained thereby. At 40° C. the coëfficient of linear expansion in the direction of the axis is α, at right angles to the same α', cubical expansion $c = \alpha + 2\alpha'$, Δ the variation for each degree; t is the temperature at which the body has a *maximum of density*. The unit of α, α', c and Δ is 0·00000001, or *one hundred millionth* of the unit of length or volume.

	α	Δ	α'	Δ	c	Δ	t° C.
Platinum with 0·1 iridium,	883·847	0·7588	....	....	....	....	.....
Mirror-glass, ...		.....	....	....	2331	4·74	.....
Diamond, ...		.....	....	....	354	4·32	−42°·3
Cuprite, ...		.. ..	....	....	279	6·30	− 4°·3
Beryl, ...	106	1·14	137	1·33	380	3·80	− 4°·2
Quartz, ...	781	1·77	1419	2·38	3619	6·53	.....
Rutile, ...	919	2·25	714	1·10	2374	4·45	.....
Cassiterite, ...	392	1·19	321	0·76	1034	2·71	.....
Periclase (artificial), ...		.....	. ..	....	3129	8·01	.....
Spartalite (ZnO), ...	316	1·86	539	1·23	1394	4·32	.....
Corundum, ...	619	2·05	533	2·25	1705	6·55	.....
Hematite, ...	829	1·19	836	2·62	2501	6·43	.....

	c	Δ	$t°$ C.
Senarmontite,	5889	1·71	.....
Arsenious acid (artificial),	12378	20·37	.....
Spinel (regular system, hence $a=a'=a''$),			.....
1. Ruby from Ceylon,	1787	7·29	.....
2. Pleonast from Warwick,	1805	5·34	.....
3. Gahnite from Fahlun,	1766	5·19	.....
4. Kreittonite from Silberberg,	1750	5·31	.....

It is evident that t is found by $c-\Delta t=0$; it thus appears that the maximum of density instead of being an exception may be the rule! Fizeau has cut a needle of beryl wherewith he proposes to verify it.—*Poggendorff's Annalen*, 1866, cxxviii, 564–589. G. H.

18. *Expansion of a conductor due to the galvanic current;* by ER. EDLUND (Swedish Academy, January, 1866).—When a current passes through a conductor, the latter is heated to a certain temperature, t, above its surroundings and consequently expanded; but it may be questioned whether this expansion is due to the temperature of the conductor, or *whether it is greater*, so that the galvanic current itself produces a peculiar expansion of the conductor through which it flows. That this last is the case has been experimentally demonstrated by Edlund. This expansion might perhaps be called *galvanic;* the usual, *caloric expansion.*

He very carefully measured the actual expansion, E, of the conductor (platinum, iron and brass were used) and the corresponding resistance R. Next he determined the expansion produced by heat alone in the same wire, so that he accurately can determine the temperature, T_1, of the conductor from E, considering this expansion due to the temperature alone. Finally, he carefully determines the influence of temperature on the conducting power of the very same wire; so that he can calculate the temperature T_2, which would correspond to the above resistance R—that is, he determines the actual temperature of the conductor while the current circulates by the resistance it offers.

Edlund found T_2 invariably from 1° to 10° *lower* than T_1—or the actual expansion, E, of the conductor is greater than would be produced by the actual heating, T_2, of the same, this being calculated from the measured resistance, R, of the same conductor. The expansion was not due to a change in elasticity, for the tension of the wire varying from $2\frac{1}{2}$ to $3\frac{1}{2}$ pounds did not change the result. *This galvanic expansion increases rapidly with the intensity of the current*—but according to what law is still unknown.—*Poggendorff's Annalen*, 1866, cxxix, 15–44.

G. H.

II. MINERALOGY AND GEOLOGY.

1. *Note on the use of the name Hudson-river group:* by F. B. MEEK.—In the Introduction to the Illinois Paleontology, just published, Mr. Worthen and the writer have some remarks on the impropriety of transferring the name *Hudson-river group*, from the older series of contorted slates and argillaceous sandstones to which it was originally applied (existing in great force along that stream above the Highlands), to the more modern group composed of the *Lorraine shales, Frankfort slates*, &c., with which the true Hudson-river rocks were subsequently confounded.

Since these remarks were in print, I observe we were in error in sup-

posing that late investigations had brought to light facts casting doubt upon the occurrence of the later types of fossils along *any* part of the Hudson river, in other than the little isolated masses alluded to as occupying synclinal axes in the older rocks, or entangled amongst their contorted strata. The fact, however, that the more modern types of fossils are not known to occur under other circumstances than those mentioned, along the part of that stream regarded as the *typical localities* of the *Hudson-river group* and lying mainly between the Highlands and the region of Albany, while the name, as originally used by Conrad and Mather, was expressly applied by them to this older series, which they regarded as belonging to the Cambrian of Sedgwick, is believed to be a sufficient reason for objecting to the transfer of the name to the later group. Hence if retained at all, it is believed this name should be applied exclusively to the group of rocks for which it was originally intended, and to which it must always carry the minds of those who may look into its origin and history. As it, however, subsequently became very generally also associated with the more recent series already alluded to, it probably could not now be restricted without much inconvenience, to the rocks to which it properly belongs. Consequently the surest, if not the only, way to avoid confusion, will be to strike it entirely from our nomenclature. The name applied to the more recent rocks, in Mr. Worthen's Report on Illinois, is the *Cincinnati* group, from the great development and highly fossiliferous character of these beds at the well-known city of that name in Ohio.*

2. *Note on Bellinurus Danæ, from the Illinois Coal-measures;* by F. B. Meek.—In the Proceedings of the Academy of Natural Sciences of Philadelphia for March, 1865, and again in the Illinois Paleontological Report, Mr. Worthen and the writer have described a new species of crustacean under the name *Bellinurus Danæ*. In both of these publications it is stated that we had not seen the original description published by the founder of the genus *Bellinurus*, nor a full description of it by any other author; but that our species, although closely allied to the forms figured by Prestwich and usually referred to this genus, differs from the characters assigned it by Portlock, Owen and some others, in having its body segments anchylosed, as well as in the position of the eyes.

In the Quarterly Journal of the Geological Society of London, Nov. 1865, p. 490, I observe Mr. Henry Woodward, in speaking of the genus *Bellinurus*, says, "the segments of the abdomen, if not anchylosed in all, are so in most" of the species.

In "The Reader" of Dec. 1866, containing an abstract of the Proceedings of the London Geological Society, it is stated that in a paper read by Mr. H. Woodward, Nov. 1866, "On some points in the structure of the *Xiphosura*," he remarked that this group is "divisible into three genera; 1st. *Bellinurus*, having 5 freely articulated thoracic segments, three anchylosed abdominal ones, and a telson; 2nd. *Prestwichia*, a new

* It has been suggested that Prof. Safford's name, Nashville group, should be retained for this formation. To this I do not seriously object: the only reason for not using it is, that Prof. S. applied it to a group including along with the so-called Hudson river rocks, the upper part of the Trenton. Hence it cannot be conveniently used when we wish to speak with precision of the later so-called Hudson river rocks, as a distinct formation from the Trenton.

genus, having the thoracic and abdominal segments anchylosed together; 3d. *Limulus*, Müller, having a head composed of 7 cephalic and one thoracic segments, followed by 5 coalesced thoracic somites bearing branchiæ, and one or more coalesced abdominal somites, to which is articulated the telson."

From this I infer, that Mr. Woodward proposes to separate as a new genus under the name *Prestwichia*, those species formerly referred to *Bellinurus*, in which the body segments are anchylosed together. If so, our Illinois species would fall into the latter group, under the name *Prestwichia Danæ*.*

3. *Section of the Rocks of Illinois, from Worthen's Geological Report*, vol. II, p. viii.

		Feet.
	POST-TERTIARY.	
	Drift, Loess, etc.—Clay, sand, pebbles, boulders, etc.....	150
	TERTIARY.	
Eocene period?	Clays and greenish sand,.....	150
	CARBONIFEROUS SYSTEM.	
Carboniferous period.	Coal-measures and Millstone grit.—Coal, shale, clay, limestones, sandstones and conglomerate,.....	1200
Mountain Limestone or Subcarboniferous period.	Chester group.—Limestone, sandstone and shale, ...	800
	St. Louis beds.—Limestone and shale, ...	200
	Keokuk group.—Limestone and shale, ...	150
	Burlington group.—Coarse, subcrystalline limestone, ...	200
	Kinderhook group.—Shales, limestone, sandstone, etc.	150
	DEVONIAN SYSTEM.	
Hamilton period.	Genesee division.—"Black slate" and grayish shale,.....	100
	Hamilton beds.—Dark grayish fetid and lighter, more pure limestones,	120
Upper Helderberg period.	Corniferous and Onondaga beds.—Gray, more or less sandy limestone,.....	25
Oriskany period.	Oriskany—upper bed.—Quartzose sandstone, ...	40
	Oriskany—lower beds, or Clear-creek group.—Highly siliceous, very cherty magnesian limestone, usually in thin layers, ...	200
	UPPER SILURIAN.	
Lower Helderberg period.	Lower Helderberg group, (D. shaly limestone of N. Y. geologists).—More or less magnesian and argillaceous limestone, in thin layers, including flinty concretions, ...	200
Niagara period.	Niagara group.—Magnesian and argillaceous limestones,...	200
	LOWER SILURIAN.	
Cincinnati period.	Cincinnati group.—Limestones, shales and sandstone,...	140
Trenton period.	Galena and Trenton beds.—Magnesian and more or less pure limestones, ...	300
Potsdam or Primordial period.	St. Peter's division.—Pure quartzose sandst.,	150
	Calciferous division.—Magnesian limestones and sandstones,.....	120 seen.

* Since the publication of the Illinois Report, I observe Quenstedt figures, on tab. 21, fig. 7, of his Handb. der Petref., under the name *Gampsonyx fimbriatus*, a little Crustacean from the Coal-measures of Germany, almost certainly congeneric with an imperfect specimen referred by us, provisionally, to our *Palæocaris typus*. (See Ill. Rep., ii, pl. xxxii, fig. 5*a*.) If Jordan's original figure, however, of the type of the genus *Gampsonyx*, as reproduced by Pictet, and that given by Bronn, are even nearly accurate, the typical specimen of our genus *Palæocaris* must be very distinct from *Gampsonyx*.

4. *Geological Survey of Canada.—Catalogue of the Silurian Fossils of the Isle of Anticosti, with descriptions of some new genera and species;* by E. Billings, Paleontologist, F.G.S. 93 pp., 8vo, with 42 wood-cuts. Montreal, Nov., 1866.—The whole number of species enumerated and described in this valuable memoir is about 330, of which some 130 are here for the first time made known to Science. Altogether they represent over 70 genera of *Protozoa, Radiata, Mollusca,* and *Articulata.* Of these genera, the following are new, viz., *Ischyrinia,*—a genus of bivalves apparently allied to the *Trigoniidæ; Streptoceras,* a Cephalopod like *Ormoceras* but with a trilobate aperture like *Phragmoceras,*—and *Sœrichnites,* supposed to be the tracks or trails of a Mollusk. In enumerating the species previously described, full references are given to the works in which the descriptions were originally published; while the larger portion of the new species are illustrated by good wood-cuts, printed in the text.

In regard to his genus *Pasceolus,* published some time back, the author says he thinks the evidence upon which it has been referred by others to the *Cystidea,* not conclusive; and he denies that he had, as some have thought, regarded it as presenting Ascidian affinities. He also doubts its identity with *Cyclocrinus* of Eichwald.

On pages 75 to 82, he has some highly interesting and instructive remarks on the paleozoic rocks and fossils of Anticosti and New Brunswick, and their relations to those of Canada, New York and Great Britain. It would be impossible to give here, even an intelligible abstract of the whole of these remarks; but it is worthy of note that he places the *St. John's group* of New Brunswick, consisting of about 3000 feet of Black slates and Sandstones (conformably underlaid by a series of rocks very like the Cambrian) *beneath the Potsdam sandstone,* and on a parallel with the Lower Lingula flags of Wales. He mentions seeing, from the St. John's group, species of *Orthis*? *Paradoxides, Conocephalites, Arionchlus, Microdiscus* and *Agnostus.*

The Potsdam sandstone he considers divisible into lower and upper, the first or lower division being represented by the sandstones and limestones of the north shore of the Straits of Belle-isle, and the rocks, which in Vermont are called the *Georgia slates,* and Red sandstone characterized by *Olenus Vermontana, O. Thompsoni, Conocephalites Adamsi,* &c., also probably including the St. John's slates (of Jukes) Newfoundland, and the Paradoxides beds near Boston.

The upper Potsdam he views as being composed of the Minnesota and Wisconsin trilobite beds, and probably the upper part of the Potsdam sandstone of Canada and New York.

The Calciferous he likewise divides into lower and upper, the first being the original typical Calciferous beds, and the latter (not known in Canada or New York) occurring in Newfoundland, where it is over 1000 feet in thickness.

Above the latter he places the Lévis formation, and next the Silery. Between the Lévis and the Calciferous formations, there is a great paleontological break, as there is also between the Lévis and the Chazy above, many of the *Trilobites* in the Lévis being closely allied to those of the Upper Lingula flags and Tremadoc slates.

This work will be indispensable to those wishing to study the rocks and fossils of Anticosti, as well as highly interesting, from the general bearing of some of the author's views, to all engaged in investigating the Silurian rocks everywhere.

5. *Tertiary of North and South Carolina;* by T. A. Conrad. (From a letter to one of the editors, dated Philadelphia, Jan. 31, 1867.)—The Tertiary strata of North and South Carolina are well worthy of a more thorough study than they have yet received. Prof. Tuomey has described some disturbed Eocene strata in South Carolina, in which he finds many specimens of the following Cretaceous forms: *Ammonites placenta*, *Terebratula Harlani*, *Gryphæa mutabilis*, and *Spondylus greyalis*. He remarks that the beds containing these fossils are of the same age as the conglomerate at Wilmington, N. C. That this mixture of Cretaceous and Eocene species is accidental is clearly proved by their never having been found in the undisturbed strata of older Eocene in Alabama and Mississippi, and especially by the occasional presence of Cretaceous fossils in the Miocene of Cape Fear river, N. C. No one would believe that they lived through the Eocene period and escaped alive into that of the Miocene. Prof. Emmons remarks that *Belemnitella mucronata*, *Exogyra costata*, and *Cucullæa vulgaris* (cast) are all found in the Miocene, and he regards their presence as accidental.

The *Ostrea Georgiana*, according to Tuomey, is associated, near Aiken, S. C., with *O. Alabamiensis*, which is a species of the siliceous sand stratum of Claiborne. The buhrstone of South Carolina and Georgia, by their fauna, would appear to be synchronous with the above-mentioned sand group of Alabama. The *O. Georgiana* may be of this age, or immediately succeeding it, but does not occur in the older Eocene, though it might be inferred from some accounts of Shell Bluff that were associated. Mr. Ruffin has published a diagram of Shell Bluff, but unfortunately he does not mention in what stratum he found *O. sellæformis*, but he remarks that *O. Georgiana* "is seen in the upper part of the perpendicular face of the cliff, above the marl of the great Carolina bed" (to which *O. sellæformis* belongs) "and there separated from it by a body of from four to ten feet in thickness. . . . This complete separation should forbid the belief that the deposit of gigantic oyster shells belongs to the same geological formation as the great Carolinian bed below."

Whatever position in the Eocene may be assigned to *Ostrea Georgiana*, it can have but one horizon throughout its vastly remote localities, and appears everywhere to have been suddenly introduced into the Eocene fauna, and to have had a short existence compared with *O. sellæformis*, as a deposit of six feet is the greatest thickness recorded.

6. *On Human remains in Belgium;* by Mr. Dupont. (Continuation of the account on pages 121, 122, of this volume.)—Mr. Dupont has very recently explored three other caverns in the valley of the Lesse; bringing up to twenty-two the number of those which he has examined in the vicinity of Dinant, since the commencement of his researches, undertaken at the expense of the State upon the recommendation of the Academy. These three new caverns bear the names of Cave of Praule, Cave of the Germans, Cave of the Nutons of Gendron. If they have furnished few new facts in regard to the study of the quaternary period,

they have presented facts important for the understanding of the relations between the human race which inhabited the country at the period of the reindeer and that which succeeded it. We will say a few words concerning the results obtained in these new explorations.

The Cave of Praule is situated five hundred meters above Furfooz, on the left bank of the Lesse, about thirty meters above the river. Breadth of the cavern, six meters; length, three and one-half meters in the middle; mean height, two meters. On the sides are two narrow galleries having a depth of four and of eight meters respectively. The quaternary sediments there were of small thickness; they showed at the bottom, immediately above the limestone floor, a thin layer of the sandy-argillaceous stratified deposit with rolled pebbles and gravel arranged in seams not continuous; in a word, the most decided characters of the lehm or middle period of the quaternary formation of the province. That deposit contained a humerus and a canine of a great bear. The layers superimposed upon the preceding are, as ordinarily, yellow pebbly clay; they are less than a meter in thickness; they contained, especially at the bottom, bones and some worked flints. The bones indicate the following species: bear, wolf, fox, horse, reindeer, goat. The flints, all worked in the knife form, are few, and are derived from the cretaceous deposits. These facts confirm the placing of the remains of the reindeer period in the pebbly clay, and the priority of that age to the deposition of that immense layer of clay. The small number of bones and of worked flints indicates a brief residence of man in that cavern, which seems nevertheless to have been perfectly adapted for habitation, being easy of access, spacious, well lighted, and very dry at that period, as is shown by the absence of stalagmites between the lehm which then covered the floor of the cavern, and the pebbly clay which is of a date subsequent to man's residence there.

The cave of the Germans is not so much a cavern as a simple shelter furnished by a dolomitic rock (stratum III. of the Carboniferous limestone), which overhangs, and under which the gypsies still establish themselves during their journeyings; whence the name which has remained to it, the gypsies being called Germans in that district. Shelters of this nature are quite numerous on the Lesse, and they are all known under the name of caves of the Germans. The following is the section of the one in question, which is situated near the road from Hulsonniaux to Celles: 5, at the level of the Lesse, rolled pebbles, transported from Ardennes, consolidated with gravel; 4, above, yellow clay with dolomitic pebbles; 3, Loess; 2, recent alluvium, formed of little seams alternately sandy and argillaceous like the lehm; 1, the same, modified and mixed with vegetable remains; in this last bed, which forms the present floor of the cavern, are found some flints of the knife form and a small hatchet of sandstone, polished and irregular in surface.

The third cavern, called the cave of the Nutons of Gendron, is situated two thousand five hundred meters (in a straight line) above the caverns of Furfooz, on the left bank of the Lesse; but as the river describes numerous curves between the two localities, the real distance is more than eight kilometers. This cavern is at an elevation of about seventy

meters from the river, near the village of Gendron: it is excavated in the *psammite* formation of Le Condroz. That is a schistose formation with some calcareous beds alternating. The limestone has produced in this cavern stalactites, which have cemented the schistose beds together, and have preserved it from the caving-in which is very frequent in caverns of this sort. Width of the cavern at the entrance two and one-half meters; length, fourteen meters; it terminates in the form of a wedge. When Mr. Dupont visited it for the first time, he was able to penetrate only to a distance of eight meters; at that point the passage was obstructed by little columns of stalactite and cones of stalagmite. The soil is formed of a sort of mold apparently derived from the decomposition of leaves, and resembling the detritus known under the name of heath-mold (terre de bruyère). It rested on a layer of yellow clay with pebbles of schist, without ornaments or remains of industry, itself resting upon the rock. The soil covered the clay only from a point eight meters from the entrance, and was itself covered by stalagmite, which attained, in some places, a thickness of six decimeters.

In examining this soil, a great number of human bones were found, which were recognized as belonging to seventeen skeletons. Although the bones were all broken, the laborers were able to observe that the remains of the head, then those of the arms and trunk, then those of the legs, were discovered successively over a length of less than two meters. They discovered afterward, in the same order, a second, a third, then a fourth row of imperfect skeletons. Afterward a little skeleton was discovered laid transversely. Then the longitudinal arrangement of the bones again presented itself; the remains of the head toward the entrance, those of the limbs toward the inmost part of the cave. Another little skeleton was found, also placed transversely, and finally two others extended, like those of the preceding rows, parallel to the axis of the cavern. Among the human bones were found bones of foxes, badgers, hens, &c. At the entrance of the cavern was found a very small fragment of cretaceous flint in the form of a plate, with three fragments of coarse pottery. Upon the slope of the escarpment, immediately under the opening of the cave, lay two large plates of schist differing from that of the cave and of the surrounding country: one of these plates measured 1·50 meters in length by 85 centimeters in breadth; the other 1·05 meters by 1·65.

After the comparative examination which was made by Mr. Bruner-Bey of the human remains found in this cavern, one may say, in general, that none of the bones differs notably from the type of the reindeer period. As during that age, there are two forms of the lower jaw, one with horizontal branches very low and stout, the other with branches more elevated and more slender. Nevertheless, the genian eminence, triangular in the external aspect, and the bifid genian eminences in the interior are here well marked, although the chin, always rounded, is yet nearly vertical. As in the jaws of the reindeer period found at Furfooz, the angle of the chin, in the pieces where it exists, is rounded, turned inward, and very much inclined; the condyle and the glenoid cavity have the same characters in the series from Furfooz and from Gendron. The molars found have generally the normal size, and the wearing of the crown

is circular in all. In one jaw with very high horizontal branches, the two first molars are of equal size: that is a fact which is observed also in the jaws of the reindeer period of Furfooz. The molars have likewise only four tubercles. Finally, in two pieces there exists also a strongly marked symphyseal prognathism. One jaw of an old man presents, besides this considerable prognathism, genian eminences much blunted, but the chin is prominent. The most perfect fragment of the upper jaw is orthognathous. Except the middle incisors, the teeth are small. Among the separate teeth is noticed a very stout canine. Two temporal bones belonging to individuals not of the same age, also exhibit perfectly the peculiarities of the type of the reindeer period; the mastoid processes are short and rounded; the glenoid fossæ are narrow and deep; the post-mastoideal part is very thick, and the depressions of the inferior cerebral lobes on the inner surface are deep. This last peculiarity is noticed likewise on a fragment of a frontal bone of the ordinary thickness; it bears all the characters of the ancient race: external orbital process and root of the nose very broad; superciliary arches very prominent; glabella triangular and depressed. A femur, scarcely epiphysized, is forty-two centimeters in length; its circumference below the trochanter minor is eleven centimeters; the length of the neck is twenty-two millimeters; the circumference is one hundred and two millimeters; it exhibits, moreover, the double curvature peculiar to the race of Furfooz, the very large trochanter minor; the rough line is very prominent, although flattened.

From what has been said above, it is seen that the Cave of the Nutons of Gendron was a sepulchral cavern. But when did it serve that purpose? This is what Mr. Dupont says on that point.

The bones and the earth which encloses them are above the yellow pebbly clay. Now, in the province of Namur, the remains of the reindeer period are always found beneath the yellow pebbly clay. This rule is confirmed by each new exploration, without having offered as yet a single exception. The superposition of the ossiferous soil above the pebbly clay demonstrates then that the skeletons in question are of date subsequent to the reindeer period. It remains to endeavor to refer them to a definite time within the period limited on the one side by the deposit of pebbly clay, and on the other by the present time. The mode of burial indicates great antiquity; it can scarcely be met with except in the dolmens.

We have said that three fragments of coarse pottery were found at the entrance of the cavern. Mr. de Mortillet, who has examined them, describes them as follows:

"Two of the fragments evidently formed a part of the same vessel, and traces are seen of the bourrelet which formed the opening; the other is red on one side, black on the other. This pottery was made by hand, without any use of the wheel. It is very poorly baked, and consequently has not passed through the oven. Powdered calcite was mixed with the clay, to give it consistency, and prevent it from cracking while drying and especially on the approach of fire, the baking being done probably before the vessels were perfectly dry. The outer surface of the fragments shows a polishing which has left numerous little striæ, as if it had been done with a bunch of grass or straw dipped in a barbotte (fine

clay suspended in a large quantity of water). These three fragments offer then all the characters of very ancient pottery. In material, in composition, they seem to belong to the plastic art of the dolmens; but the border which surrounds the opening on two of them and the application of a barbotte would perhaps prove them to be a little more recent. It seems then more natural to refer them to the period of transition between the stone and the bronze. We are thus brought, says Mr. Dupont, to consider this race as the same that constructed those fortresses with cyclopean walls of which numerous examples are found in the province, and as belonging to the same age with the celebrated remains collected at Chauvaux by Mr. Spring."

In regard to the origin of the humus or soil which covered the skeletons, it is quite difficult to explain. It is certain that the existence of the humus, and consequently the introduction of the leaves which formed it, is more ancient than the layer of stalagmite which covers it, a layer which attains, in certain places, a thickness of six decimeters. It is certain also that, since the formation of the layer of stalagmite, no more humus has been formed, for the stalagmite contains none and is covered by none. It follows from this that a long space of time must have passed since the introduction of the leaves, for the rock in which the cavern is found is formed of schist, in which the limestone appears only in rare beds fifteen or twenty centimeters in thickness. This last remark agrees then with the preceding in attributing to the burials a very remote antiquity. If now one recalls the discovery, at the entrance of the cavern, of large plates of schist, not derived from the walls of the cavern, nor from the surrounding country, and consequently evidently brought from elsewhere, one would be tempted to see in these plates the fragments of a flag-stone which had closed artificially the entrance of the cavern, and had been subsequently removed and broken. If such was the case, the presence of the soil or rather of the decomposed leaves under the stalagmite would be explained with difficulty by a natural introduction. Perhaps it might be better explained by supposing, for burials of that remote period, a custom still used by the Peaux-Rouges of Brazil and the New-Caledonians, a custom which consists in wrapping corpses in a layer of leaves, and placing them thus enveloped in caves, in dolmens, and hanging them upon trees. Admitting this supposition, we should understand why dolmens are so rare in the province of Namur, numerous caves presenting themselves in many places to the men of that age to serve exactly the purpose of dolmens, and exempting them from erecting monuments whose construction must then have required great labor. We are thus led to consider sepulchral caverns as natural dolmens, which were used in the period of the reindeer or in the period of polished stone.

7. *Volcanic eruptions in Hawaii;* by Rev. T. Coan. (From a letter to J. D. Dana, dated Hilo, Hawaii, Aug. 31, 1866.)—I wrote you Feb. 27th of an eruption in Mokuaweoweo on the summit of Mauna Loa. This was first noticed at Hilo about the last of Dec. 1865, and we continued to see the light and smoke until the last of April, or four months. I am told by Mr. Richardson, who keeps a good hotel at Kilauea, that from his place he occasionally saw steam rising from that crater during all

May. It now seems quite extinct, and though the action for two months was vigorous and the light vivid, the lavas never overflowed the rim of the great crater or burst out laterally. Like most of the eruptions in Kilauea, the action was confined within the walls of the old crater.

In May, June, and July, the action in Kilauea was greatly increased. It was often intense and vehement. The old south lake (Halemaumau) overflowed several times, and a chain of lakes, three, four, and sometimes five or six, opened on a curved line from N.W. to N. and N.E. from the old lake. The action in this chain of lakes was often violent. Jets of lava were thrown 50, 100 and 200 feet high; the lakes overflowed, and fiery rivers seethed along the northern and eastern walls of the crater, sweeping around beyond the eastern sulphur banks. This curved line of action is about four miles long, and the igneous stream was in some places half a mile wide. The new deposits lie in strata of 50 to 100 feet in thickness. Cones and domes of lava were also raised, and yawning fissures opened, interrupting the traveller in crossing the bottom of the crater. At different times, and sometimes for many days, the fiery flood swept up to the path by which visitors go into the crater and cut off all ingress by the usual route. Many parties were obliged to view the surging waves from above, without being able to enter the crater. Occasional earthquakes shook down avalanches of rocks from the walls of the crater, and frightened the spectators.

During all this action, extending more than half round the crater, the central area, an elevated plateau, remained undisturbed, unless it may have been raised quietly, which is probable, by the forces below. This central table has been, for years, 200 feet higher than the surrounding area between this and the outer walls. For a few weeks past the action in Kilauea has been feebler, but we have no assurance that it will not increase at any time.

There has been a vast filling up and an upraising in Kilauea since 1840. Should you now visit it, you would recognize nothing except the outer walls and the surrounding regions. Internally *all* is changed and all is new. The lavas now stand higher in the crater than before the great eruption of 1840. Whether the walls will sustain the pressure until the vast pit fills and overflows the outer rim, or whether they will be rent and give lateral vent to the fusion, as in 1840, remains to be seen. And whether the cessation of action on Mt. Loa, about the last of April, had any influence on the increased action in Kilauea in May, June and July, we leave for the geologist to determine.

8. *Notice of a Human Skull, recently taken from a Shaft near Angel's, Calaveras County;* by J. D. Whitney.—This skull was taken from a shaft sunk on a mining claim at Altaville, near Angel's, in Calaveras county, by Mr. James Matson. By him it was given to Mr. Scribner, of Angel's, and by Mr. Scribner to Dr. Jones. Mr. Matson states that the skull was found at a depth of about one hundred and thirty feet, in a bed of gravel five feet in thickness, above which are four beds of consolidated volcanic ash, locally known as "lava;" these volcanic beds are separated from each other by layers of gravel, and Mr. Matson gives the following as the section of the various deposits passed through in sinking the shaft, which is one hundred and fifty-three feet deep, to the bed rock:

1.	Black lava,	40 feet.
2.	Gravel,	3 "
3.	Light lava,	30 "
4.	Gravel,	5 "
5.	Light lava,	15 "
6.	Gravel,	25 "
7.	Dark brown lava,	9 "
8.	Gravel,	5 "
9.	Red lava,	4 "
10.	Red gravel,	17 "
		153 feet.

The skull was found, according to Mr. Matson, in bed No. 8, just above the lowest stratum of lava. With the skull were found fragments of silicified wood, the whole being covered and partly incrusted with stony matter, so that the fact of its being a skull was not recognized until after it had passed into Mr. Scribner's hands, by whom it was cleaned and presented to Dr. Jones.

The skull is said by Mr. Matson to have been taken from the shaft February 25th, 1866, and it came into my hands in the July following, when I immediately proceeded to the locality, but found the shaft temporarily abandoned and partly filled with water, so that it was impossible at that time to make any farther search in the bed from which the skull was procured. A careful inquiry into all the circumstances of the alleged discovery, and an interview with all the persons who had been in any way connected with it, impressed upon my mind the conviction that the facts were as stated above, and that there was every reason to believe that the skull really came from the position assigned to it by Mr. Matson. Still, as it is evidently highly desirable that as large an amount of evidence as possible should be accumulated in regard to a discovery of so much importance, I made arrangements that I should be notified whenever the shaft was reopened and the water taken out, and hope at a future meeting to be able to lay before the Academy the results of a personal examination of this interesting locality, and of further excavations in the bed from which the skull was taken.

Assuming the correctness of Mr. Matson's statements, this relic of human antiquity is easily seen to be an object of the greatest interest to the ethnologist as well as the geologist. The previous investigations of the Geological Survey have clearly demonstrated the fact that man was contemporaneous with the mastodon and elephant, since the works of his hands have been repeatedly found in such connection with the bones of these animals that it would be impossible to account for the facts observed on any other theory. (See Geology of California, vol. i, p. 252.) But in the case of the skull now laid before the Academy, the geological position to which it must be assigned is, apparently, still lower than that of the mastodon, since the remains of this animal, as well as the elephant, which are so abundantly scattered over this State, are always (so far as our observations yet extend) limited in their position to the superficial deposits, and have never been found at any considerable depth below the surface. There is every reason to believe that these great proboscidians lived at a very recent date (geologically speaking), and posterior to the epoch of the existence of glaciers in the Sierra Nevada, and also after

the close of the period of activity of the now extinct volcanoes of that great chain. In fact, they belong to the present epoch. The bed, on the other hand, in which this skull was found, must have been deposited at a time when the volcanoes of the Sierras were still in vigorous action, and, as seems to us highly probable from a careful consideration of the geological structure of the region, previous to the glacial epoch of the Sierra, and also previous to the erosion of the cañons of the present rivers. No pains will be spared, however, to investigate all the conditions of the occurrence of this skull, and they will be fully reported on at a future time.

The portions of the skull which are preserved are, the frontal bone, the nasal bone, the superior maxillary bone of the right side, the malar bones, a part of the temporal bone of the left side, with the mastoid process and the zygomatic process, and the whole of the orbits of both eyes. The base of the skull is imbedded in a mass of bone breccia and small pebbles of volcanic rock, incrusted with a thin layer of carbonate of lime, which appears once to have extended over the whole surface of the skull and of which a considerable portion still remains, the rest having been removed apparently in the process of cleaning. Under the malar bone of the left side, a snail shell is lodged, and partly concealed by the breccia of bone wedged in the cavity. This shell is the *Helix Mormonum*, according to Dr. Cooper, a species now living in the region where the skull was obtained. Although not competent to express a decided opinion on the subject of the ethnological relations of this skull, I should suppose that it belonged to the type of the Indians now inhabiting the foot-hills of the Sierra. It is certain that the facial angle is not one indicating a low order of intellect. The skull, however, seems to have been very thick and solid. It will be placed in the hands of competent craniologists for examination and description, as soon as reliable information has been obtained with regard to its occurrence, or whenever all has been ascertained that can be.—*Proc. California Acad. Nat. Sci.*, iii, 277.

9. *Notice of the occurrence of the Silurian Series in Nevada;* by J. D. Whitney.—At a meeting of the Academy in May last, I gave some account of the geology of the state of Nevada, with particular reference to the age of the stratified deposits occurring there, as determined from the collections of fossils brought from that region to the office of the Geological Survey, by J. E. Clayton, and various members of our corps. In that communication I spoke of the probable future discovery of rocks older than the Carboniferous or Devonian, in the mountain ranges near Austin. This expectation has been realized, and we are now in possession of a very interesting collection of fossils, obtained by Mr. A. Blatchley, in the vicinity of the Hot Creek Mining District, about one hundred miles southeast of Austin. This collection enables us to state positively that both Upper and Lower Silurian rocks occur in that district, and that they are well filled with fossils; not less so, indeed, to judge from the specimens received, than the strata of the same age in New York, Ohio, Iowa, and Wisconsin, which they resemble in a most marked degree, both lithologically and paleontologically.

The fossils from the Hot Creek district are mostly weathered out on the surfaces of thin slabs of bluish-gray argillaceous limestones, and are

crowded together in the same profusion with which they have often been noticed by myself and others as occurring in the Lower Silurian shales and limestones of the Wisconsin lead region, around Big Bay des Noquets, and in many other localities in the country bordering on the great lakes.

Both the upper and lower divisions of the Silurian appear to be represented by the fossils of the Hot Creek District; but the Lower Silurian seems to be much the most prolific in fossils, as is the case in Wisconsin and Iowa. The particular period to which these Lower Silurian forms may be referred is the Trenton, including the Chazy, Birdseye, Black river and Trenton limestones of the New York geologists, and the Buff and Blue limestones of the western surveys. Nearly all the prevailing types of the eastern rocks of this age are represented in the Hot Creek collection, namely, Brachiopods, Gasteropods, Cephalopods, Crinoids, Trilobites, and Corals; and there are among them several of the most widely-distributed and most characteristic species of the Lower Silurian. The following have been identified: *Maclurea magna*, a characteristic Chazy species, and *Pleurotoma lenticularis*, *Orthis testudinaria* and *Chætetes lycoperdon*, all of which are abundant in the Trenton limestone of New York, and the rocks of the same age farther west. Among the fragments of Trilobites, two or three different genera may be recognized, especially *Asaphus*, which is represented by a species apparently new. There are also fragments of Crinoids or Cystids closely resembling the species figured by Hall, in the Paleontology of New York, vol. i, as *Echino-encrinites anatiformis*.

The rocks containing the above-mentioned fossils crop out in the sides of a deep cañon; and overlying them, at a perpendicular distance of about a thousand feet, is a series of beds containing numerous fragments of corals and crinoids, silicified and weathered out from the surface of a bluish-gray limestone, which I refer without much doubt to the age of the Niagara limestone of New York. Among the corals, *Heliolites spinipora* and *Syringopora* are recognizable; and among the crinoidal fragments are stems of what appears to be *Caryocrinus ornatus*.

With the exception of the Potsdam sandstone fossils, described by Meek and Hayden as occurring at the base of the fossiliferous series, in the Black Hills, no recognizable Silurian forms have been observed by geologists, in the Rocky mountains, or anywhere to the west of them, unless possibly in New Mexico. The Silurian series, with the possible exception of the Potsdam sandstone, seems to be entirely wanting in the Rocky mountains proper, the Black Hills being a sort of outlier of the main ranges, and lying as far east as the one hundred and third to the one hundred and fifth meridian. Dr. Hayden says, in his paper, on the Geology and Natural History of the Upper Missouri, published in 1862, that "hitherto no indications of the existence of any other member (than the Potsdam sandstone) of the Silurian period have been discovered along the eastern slope of the Rocky mountains within the boundary of the United States." He considers it probable that the Potsdam sandstone is represented in the Rocky mountains, although no fossils of that member of the series have been as yet discovered anywhere to the west of the Black Hills.

On the Mexican Boundary Survey, a few fragments of fossils were found in the superficial detritus, near El Paso (longitude 106°), which indicated the existence of Silurian rocks in that vicinity; but none appear to have been found in place. Professor Hall remarks that "the specimens referable to strata of this age (Devonian and Silurian) are few, and they are in such condition as to give little satisfactory information regarding the rocks in place." The specimens obtained are figured in the Mexican Boundary Report, but not described, nor is their locality accurately stated.

Dr. Newberry, in his Report on the Geology of the Colorado river region, refers the lower portion of the strata exposed in the grand cañons of that river to the Devonian and Silurian series; but as no recognizable fossils were discovered by the Ives expedition from any rocks lower than the Carboniferous, this reference can only be taken as expressing a conviction based on lithological characters and stratigraphical considerations.

In view of the facts above cited, it will be seen at once how interesting this discovery is of undoubted Silurian rocks west of the Rocky mountains; and the more so, since we have in this remote region a recurrence of conditions and forms of animal life so closely allied to those with which we are familiar in the states east of the Mississippi. It is a very remarkable fact that these rocks have not been discovered in the Rocky mountains; and should farther explorations fail to reveal their presence, it will throw a new light on the history of the physical development of the central and western portions of this continent. Taking into view what has now been communicated, and what was stated in my previous paper in regard to the existence of the older stratified rocks in the Silver Peak District, it will appear that Dr. Newberry's generalizations were, in all probability, correct, and that we may expect to find in southern and southwestern Nevada the outcropping fossiliferous edges of the strata underlying the Carboniferous of the great Arizona or Colorado plateau, and that they will be proved to occupy an extensive area, and to yield a profusion of organic remains.

Among the specimens collected by Mr. Blatchley, as also by Mr. Clayton, Mr. Melville Attwood, and Dr. C. L. Anderson, and now at our office, there is a considerable number which demonstrate the existence of an extensive freshwater Tertiary deposit in Nevada. This formation, which belongs to a very late Tertiary epoch, evidently occupies a considerable area, as our specimens come from localities hundreds of miles distant from each other. The existence of any marine formation more recent than the Jurassic, in Nevada, has not yet been proved; but, as Mr. Gabb obtained evidence, in 1864, of the occurrence of rocks of Cretaceous age on Crooked river, in Oregon, east of the Cascade Range, it is possible that this member of the series may yet be discovered in Nevada.

All the fossils referred to in this and my previous communication on the geology of Nevada, will receive, in due time, thorough investigation at the hands of Messrs. Meek and Gabb, or other competent paleontologists; and we expect that our collections from that state will be largely increased during the present year.—*Proc. California Acad. Nat. Sci.*, iii, 307.

10. *Eozoön.*—At a meeting of the Natural History Society of Montreal in January last, Dr. Dawson exhibited a photograph of a remarkable specimen of *Eozoön Canadense*, found the past summer in the Laurentian limestone of Tudor, Canada West, by Mr. Vennor, of the Canadian Geological Survey, and which had been examined and described for Sir W. E. Logan by Dr. Dawson. The rocks at Tudor and its vicinity, which, according to the observations of Mr. Vennor are Lower Laurentian, have experienced less metamorphism than is usual in formations of that age, and this peculiarity gives especial interest to the present specimen, which is contained in a rock scarcely altered and in a condition not essentially different from that of ordinary Silurian fossils.

The matrix is a coarse laminated limestone of a dark color, and containing much sand and finely comminuted carbonaceous matter. The fossil itself is of a flattened clavate form, about six and a half inches in length, and with the septa of its chambers perfectly preserved, exhibiting on one side a well defined marginal wall produced by coalescence of the septa, and apparently traversed by small orifices. Under the microscope the minute structures of *Eozoön Canadense* can be detected, though less distinctly perceived than in some of the specimens mineralized by serpentine. In some of the chambers there are small amorphous bodies containing pointed siliceous spicules, which seem to be the remains of sponges that have established themselves in the cells after the animal matter of *Eozoön* had disappeared.

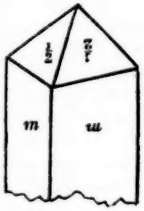
The importance of this specimen was pointed out as establishing the conclusions previously arrived at from the study of the remains of *Eozoön* included in the serpentinous limestones, and as overthrowing the objections raised in some quarters to the organic origin of *Eozoön*. The specimen will be taken to England by Sir W. E. Logan, and full details of its characters will be communicated to the Geological Society, along with some other recent discoveries tending to the establishment of a second species of *Eozoön.*—*Montreal Guzette*, Jan. 29, 1867.

11. *On Fossils in the Auriferous rocks of California;* by W. P. Blake, (from a notice of the meeting of the California Academy, Aug. 20, 1866.) —Prof. Blake read a paper upon a "New Locality of Fossils in the Gold-bearing Rocks of California," and exhibited specimens of Ammonites from a cut on the line of the Central Pacific Railroad, near Colfax. Although the specimen exhibited was quite perfect, it was not sufficiently so to enable the septæ to be seen. Prof. Blake remarked that these fossils were undoubtedly of the secondary period, and that they were apparently specifically identical with those from the American river, in the same vicinity, of which he had sent photographs to Mr. Meek, in 1863, and afterwards noticed the same at a meeting of the Academy in September, 1864; he also thought them identical with the species found at Bear Valley, Mariposa county. Prof. Blake also exhibited the tooth of an extinct elephant, a *molar*, found in the auriferous gravel near Michigan Bluffs; also, shark's teeth and other marine remains from Tulare valley; these remains were found by him at an elevation of at least twelve hundred feet above the sea, and probably belong to the Post-pliocene period.

Prof. Blake alluded to a "Quarry of Gold-bearing rock" in Placer county, known as the Banker or Whisky Hill mine, where no regular

quartz vein exists, and the rock of the hill is profitably worked; some forty tons per diem are crushed, yielding from two to twenty dollars per ton—averaging from five to six dollars per ton.

12. *On the Crystalline form of Pachnolite;* by A. DESCLOIZEAUX. (From a letter to J. D. Dana, dated Dec. 6, 1866.)—Pachnolite is not quadratic as has been stated. Its crystals are much less simple. They belong to the monometric system, and appear to me to be always macled, in the same manner as are those of amphibole. As only a single summit is visible, the crystals being retained in the gangue by the opposite one, it seems as if we were dealing with a right rhombic prism. The four faces of the terminal octahedron are actually of the same kind, and belong to a positive hemi-octahedron, $b\frac{1}{2}$. The crystals therefore appear as in the annexed figure, the axis of revolution being normal to the plane bisecting the acute angle of the fundamental prism. We may admit as a mean—



		Calculated.	Observed.	
m on m in front,	=	98° 34′	98° 40′	
m on m lateral,	=	81 26	81 43	macle.
$b\frac{1}{2}$ on $b\frac{1}{2}$ (planes $\frac{1}{2}$ in figure) adjacent,	=	108 15	108 15	
$b\frac{1}{2}$ on $\frac{1}{1}q$ adjacent,	=	94 13	94 13	macle.
m on $b\frac{1}{2}$ adjacent,	=	153 37	153 46	
$\frac{1}{1}q$ on $b\frac{1}{2}$ opposite,	=	52 46	52 46	

Edges $\frac{b\frac{1}{2}}{b\frac{1}{2}} : \frac{m}{m}$ adjacent,	=	146° 45′
Base $p : m$,	=	90 20
Plane angle of the base,		98 33 54″
Plane angle of the lateral faces,		90 16 48

The plane of the optic axes is perpendicular to the plane of symmetry, and makes an angle of about 10° to 15° with a normal to the edge $\frac{m}{m}$, and an angle of about 23° 15′ to 18° 15′ with a normal to the edge $\frac{b\frac{1}{2}}{b\frac{1}{2}}$. One of the bisectrices is normal to the plane of symmetry; the other makes with the edges $\frac{m}{m}$ and $\frac{b\frac{1}{2}}{b\frac{1}{2}}$ the same angles as the plane of the axes: but I have not yet been able to ascertain which of these is the bisectrix of the *acute* angle of the axes. The former is *negative*. As it has been impossible to cut a single thin plate normal to the bisectrices, it has not been possible to study the dispersion of the optic axes. The crystals rest upon small, translucent, crystalline laminæ, which appear to possess different optical properties. Is this mineral arksutite? These laminæ appear to make a transition stage between cryolite and pachnolite.

III. BOTANY.

1. *Salices Europææ;* recensuit et descripsit Dr. FRIDERICUS WIMMER. Breslau, 1866. pp. 286, 8vo.—While awaiting the appearance of the general elaboration of the order, contributed by Dr. Andersson to the Prodromus, and of the *Icones Salicum* he has also prepared, we receive this beautiful volume, in which Dr. Wimmer, restricting his attention to the European Willows, has revised these with much detail and evidently with very great care. The work is written throughout in Latin. There is a full account of the literature of the subject, of the structure and morphology, and of the various systematic arrangements of the species which different authors have proposed. Dr. Wimmer himself arranges the Willows of Europe under eleven tersely characterized tribes, each comprising from one to six species. He admits only 31 to the rank of genuine species, reducing a great number to mere synonyms, and having well characterized these in the first half of his volume, he devotes the rest to *Salices hybridæ*. These hybrids, 57 in number, and chiefly named according to their parentage, are disposed under fourteen sections, and their synonymy is fully indicated. The whole work gives the impression of being thoroughly reliable and excellent. A. G.

2. *Le Specie dei Cotoni descritte da* FILIPPO PARLATORE. Florence, 1866.—Professor Parlatore's essay upon the long-vexed question of the species of Cotton was called forth by a Royal Italian Commission, and is dedicated to its President, Devincenzi. The letter-press, of 64 pages, 4to, is divided into a general history of cotton-plants, in Italian; and the description of the species, with the characters and full synonymy, are in Latin, but the farther details in Italian. He describes seven species (besides referring to as many doubtful ones): *G. arboreum*, or Tree Cotton; *G. herbaceum*, the common herbaceous Cotton; *G. Barbadense*, or Sea-Island Cotton; *G. religiosum*, or Peruvian Cotton; *G. hirsutum*, or Siamese Cotton; *G. Taitense*, as a new species, from Tahiti, which had been long variously confounded with others; and *G. Sandvicense*, also as a new species from the Sandwich Islands; but this has already been published by Dr. Seemann, under Nuttall's name of *G. tomentosum*. These species are all illustrated in a separate atlas of six plates, in elephant folio, beautifully exhibiting them in specimens of the natural size, in good lithography, all but the last two printed in colors. Our copy of this fine work is presented by the President of the Royal Commission, through the author.

Coniferæ, &c.—It is well known that Prof. Parlatore has elaborated the *Coniferæ* for the Prodromus. We have from him his study of the organography of the flower and fruit of this order, a memoir, contributed to the Annals of the Museum of Natural History at Florence, written in Italian; in quarto, with three folio plates, filled with structural details. Botanists are aware that he does not adopt the view that they are gymnospermous. We may soon expect, in the Prodromus, the full presentation of this view.

A separate little sheet contains characters, given in advance, of a few new *Coniferæ:* among them a Larch of N.W. America, *Larix Lyallii*, remarkable for a cobwebby woolliness of the young shoots and leaf-buds,

the scales of the latter with a fringed margin. We have from the same author his

Considérations sur le Méthode Naturelle en Botanique. 8vo pamph. Florence, 1863.—A good historical view of the development of the natural method, with some pertinent illustrations of the obvious importance in all the divisions, from highest to lowest, of looking to *types* and the *ensemble* of characters rather than resting upon single points,—in other words, of carrying the spirit of the natural rather than of artificial systems throughout the whole domain of botany. A. G.

3. *Tree-labels for the Arboretum.*—Mr. J. H. Creighton, of Chilicothe, Ohio, who is forming, with rare and thoughtful generosity, an extensive arboretum for a college in Ohio, has contrived a plan of permanent label for the trees, which is worthy of general adoption in public grounds and parks. The label is of cast iron; and it exhibits in capital letters, in bold relief, the scientific and the popular name of the tree. Having the pattern and a stock of letters prepared for his own use, Mr. Creighton offers to cast, at a low rate, any number of labels, with any desired name.

A. G.

4. *Ozone produced by Plants.*—Professor Daubeny of Oxford has contributed to the Journal of the Chemical Society, for January last, an interesting article, giving the details of a series of careful experiments, which go to prove that green foliage, in assimilating carbonic acid, water, &c., liberates a part of the oxygen in the form of ozone. After his experiments were made, Dr. Daubeny found that Kosmann of Strasburg had reached the same conclusion, but through less refined experiments. Referring to the first paper he ever communicated to a scientific society, that published in the Philosophical Transactions for 1834, on the evolution of oxygen gas by plants in the day-time, Dr. Daubeny concludes: "Should I now have established to the satisfaction of the scientific world, that these same green parts of plants, at the very time they are emitting oxygen, convert a portion of it into ozone, I might hope that these researches of my later years will serve appropriately to wind up those undertaken in my younger ones, by showing that vegetable life acts as the appointed instrument for counteracting the injurious effects of the animal creation upon the air we breathe, not merely by restoring to it the oxygen which the latter had consumed, but also by removing, through the agency of the ozone it generates, those noxious effluvia which are engendered by the various processes of putrefaction and decay,"—engendered, we may add, as much by decaying vegetable as by animal matter.

A. G.

5. *Morphology of Stamens, and use of Abortive Organs.*—In a recent number of the *Gardeners' Chronicle*, the editor, in giving a full account of *Dombeya angulata*,—a rare plant, which has just flowered at Kew,—describes as follows the morphology of the stamens, and the remarkable assistance which the *staminodia*, or barren stamens, seem to render as *go-betweens*, carrying pollen from the efficient stamens to the stigmas.

"The stamens in this plant, as in all the Malvales, may be looked upon as compound: while the ordinary stamen corresponds to a simple leaf, the groups of stamens in the Mallows and allied orders may be regarded as the equivalents of compound leaves, united together at their bases.

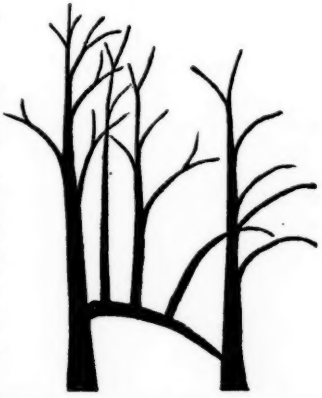
Some of the lobes or leaflets of these compound leaves bear anthers, while others are destitute of anthers, and constitute the barren stamens or staminodes. Some light is thrown on the uses of these barren stamens by an examination of the plant now under consideration. In the fully expanded flower, the inner surface of the upper angle or point of each petal is about on a level with the stigma and with the tip of the barren stamen, the outer flat surface of which latter, as well as the adjacent portion of the petal, are often dusted over with pollen, the true stamens nevertheless being at a considerable distance beneath these organs. In less fully developed flowers the barren stamens may be seen curving downwards and outwards so as to come in contact with the shorter fertile stamens, whose anthers open outwardly, and thus allow their contents to adhere to the barren stamens. These latter, provided with their freight of pollen, uncoil themselves, assume more or less of an erect position, and thus bring their points on a level with the stigma, whose curling lobes twist round them and receive the pollen from them. The use, then, of the long staminodes seems to be to convey pollen from the short fertile stamens to the stigma, which, but for their intervention, could not be influenced by it. The presence of pollen on the upper and inner corner of the petals is readily explained by the fact that owing to their position and peculiar form they all come in contact with the ends of the staminodes and the stigmas, and hence they too get dusted with pollen.

"These arrangements would therefore seem to favor self-fertilization; and they show how an organ spoken of sometimes somewhat contemptuously, as barren, rudimentary, imperfect, or the like, may yet play an important part, both in the architectural plan of the flower, and in its life-history."

We venture to suggest that, although this curious arrangement may serve to ensure a certain amount of self-fertilization, yet it must likewise be studied in relation to the action of insects which probably visit these flowers, and, we fancy, may have something to do either in the deposition or in the removal of this pollen, so bringing about cross-fertilization.

In regard to the stamineal structure, briefly sketched in the beginning of the above extract, we would remark that, if we mistake not, Duchartre should have the credit of having brought to view this structure of the malvaceous androecium more than twenty years ago; that the inferences from organogeny soon after found a striking illustration in this country in the genus *Sidalcea;* and, finally, we are gratified to perceive that a theoretical view which has in this country been set forth as a part of elementary botany for many years (see Bot. Text Book, third edition, 1850, p. 249–252), is now making its way in England. It is evident that the name (*deduplication*), and the fanciful or *un*-morphological conceptions, with which this sort of multiplication of organs (in which a pair or a group occupy the place of one) was originally associated, have prejudiced British botanists against the *thing;* and it was on this account that we preferred the unobjectionable name of chorisis,—defining it in the terms of the above parenthesis, and illustrating it, in the class of cases in question, by the comparison of such compound stamen with a compound leaf. In another class of cases, anteposed parts are likened to intrafoliaceous stipules. A. G.

6. ***Remarkable union of two trees;*** by PAYSON W. LYMAN.—Near the village of North Chester, in Hampden Co., Mass., there stands a remarkable elm tree (*Ulmus Americana*), of which the annexed sketch will give a tolerably accurate idea. It stands near the summit of a range of hills, on the eastern bank of a branch of the Westfield River, in a narrow ravine, on either side of which wooded hills rise abruptly. It is considerably exposed to the light, being near the edge of the woodland.



The two parts of the tree, which rise from the ground and support the arch, are 30 feet apart, the one on the left of the sketch being as much as 2½ feet in diameter, and rising to a height of nearly or quite 100 feet, while that on the right is 1¾ feet in diameter, and not so high. The arch springs from the tree on the left at the height of 14 feet, and connects with that on the right at the height of 4 feet. Its diameter, near its junction with the former, is fully 1½ feet, gradually diminishing in size toward its junction with the latter, where its diameter is about 6 inches.

Its union is equally perfect at either end, though its connection seems to be more natural and regular at its larger extremity; which would lead one to suppose that, in some way, a branch of the tree on the left had been bent over and become engrafted into that on the right. The probability of this supposition is enhanced by the statement of the gentleman who owned the land forty years ago, that the tree then stood on the line of a fence, and that he, noticing the arch, supposed that it had been bent over to form part of the fence. He further states that, if he remembers correctly, the branch now forming the arch then extended beyond the tree with which it has united.

The arch runs lengthwise of the ravine, and, together with the trunks which rise from it, derives its nourishment from both sets of roots. Of these three intermediate trunks, that on the left is 14 inches in diameter, and attains an altitude equal to that of the main trunk on the left of it. The diameter of the second is 11 inches, and of the third 6 inches, the two rising to a height proportioned to their size. By a rough estimate it would appear that the horizontal branch supports a weight in addition to its own of about 4400 pounds. About midway between the two original trees, there rises a beech tree (*Fagus ferruginea*), which divides before reaching the arch, which it includes within its branches, but does not in any place come in contact with or support it.

A person desiring to see this remarkable tree would need to go about a half-mile north of the village of North Chester, to a saw-mill, where he would cross to the east side of the stream, traverse the fields, and climb the hill, following a little ravine for a distance of forty or fifty rods to the edge of the woodland, where he would readily discover the tree.

It is somewhat difficult of access, but would repay any one interested in objects of this kind for the trouble required to reach it. It has been known more or less to individuals in its vicinity for forty years at least, but has never attracted very wide attention.

Amherst College, Jan. 15th, 1867.

IV. ASTRONOMY AND METEOROLOGY.

1. *November meteors in* 1866.—Accounts of the grand meteoric shower on the morning of the 13th–14th of November continue to reach us. We compile the following summary.

(*a.*) *In the Sandwich Islands* (W. long. 158°, N. lat. 21° 15′).—Mr. J. P. Cooke, with seven others, saw 317 meteors in five hours on the night of Nov. 13th–14th, 1866.

From 10h	to 11h	P.M.,	22	meteors.	Sky clear.
" 11	" 12	"	44	"	Cloudy in N.E.
" 12	" 1	A.M.,	47	"	Sky half covered.
" 1	" 2	"	63	"	Clear.
" 2	" 3	"	141	"	Clear.
Total in five hours,			317	"	

There was no special radiation from Leo. A few brilliant meteors were observed between 3h and 4h A.M., but they were not as numerous as between 2 and 3 o'clock. No count of them was kept however.

These observations serve as a continuation of those in the United States, and show that there was no revival of the shower directly after our dawn.

(*b.*) *Asia Minor.*—Rev. A. T. Pratt, M.D., in a letter from Marash, Turkey, gives the following account of the shower as seen at that place.

"After midnight, we did not look out till 2h 45m A.M., when they were decidedly frequent. Miss Spencer, who is living with us, being called, we went out and took our station to count them, with result as follows:

	N.W.	S.W.	Total.
From 3h 0m to 3h 15m,	200	298	498
" 3 15 " 3 30	300	566	866
" 3 30 " 3 45	486	662	1,148

"They were so numerous after this as to make it impossible to count them; falling by dozens and scores at a time till about 4h 30m, when from 4h 30m to 4h 40m, in the southwest, were counted 200,—the number having sensibly decreased,—when we were compelled to leave the field.

"All the meteors radiated from the same *space* in the heavens (hardly the same point), i. e., that between γ and ε Leonis, and passed thence in every direction, only two or three being observed which did not conform to this law."

(*c.*) *In India.*—Rev. Wm. Wood, in a letter to his son from Ahmednuggur (N. lat. 19° 5′, E. long. 74° 55′), says that he watched all the night of the 13th–14th, and that a good many meteors fell before midnight, and so on till three o'clock, but that from that time till light there was a shower of them. At one time he counted 100 in five minutes.*

* It is not certain, however, whether these were seen by one or by five persons.

One meteor left a train which moved *westward*, being visible for five minutes.

(*d.*) *In India.*—Rev. Edward Chester, writing from Dindigul (N. lat. 10° 25′, E. lon. 78°), says: "For a few moments I began to count the meteors, but they came so thick and so many at a time that I had to give it up. I saw thousands. I saw nothing else from 4 o'clock until daybreak, and even saw some within fifteen minutes of sunrise."

(*e.*) *In the N. Atlantic.*—Mr. E. Guillemin,* at sea 45 miles N.E. of Flores, saw the meteors cross the sky from E. to W., with long trains. A narrow cloud 10° high lay along the horizon. From behind it, from a point N. 59° E., luminous trains shot across the sky. The parallel paths were projected into the meridians of a sphere, the point of divergence being the pole. Those which were near the horizon were shorter and more like the ordinary shooting stars. Toward the zenith, however, they were much more brilliant, and their time of flight longer. Those which crossed the zenith rose perpendicularly from the eastern horizon, and leaving behind an immense phosphorescent arc, disappeared low in the west. From 10^h to 11^h the stars appeared either isolated or in groups of two and three, at intervals of from four to twenty seconds. They moved with uniform velocity, and had a mean duration of flight of seven or eight seconds. After 11 o'clock the display gradually diminished, and ended about half past three in the morning.

(*f.*) *At Cape of Good Hope.*—Mr. G. W. H. Maclear reports (Astron. Monthly Notices) between 10^h P.M. and 13^h A.M. 33 meteors seen at the Cape of Good Hope. Between 13^h 3^m and 16^h 21^m A.M. they saw 2742. Of these there were 1774 between 13^h 51^m and 14^h 36^m 45^s, that is, in about three-fourths of an hour. The number of observers is not stated.

(*g.*) *Eastern Asia.*—There appears to have been no remarkable display at Shanghai, and at Yokuhama in Japan a watch kept that night detected nothing unusual.

(*h.*) *At Athens.*—Dr. J. F. J. Schmidt deduces hourly numbers for one observer for the twelve hours from 6^h P.M. to 6^h A.M. from observations made at Athens. There were different persons actually counting during the separate hours, but allowances are made for personal peculiarities. During the two critical hours, 2^h to 4^h A.M., no direct count was kept up. The following are the numbers:

6	6	133	785 (?)
6	7	980	405
6	50	1055 (?)	125

Total in the 12 hours, 3564 meteors.

(*j.*) Throughout the continent of Europe the shower was observed, and it maintained everywhere the same general character, no differences being noted that may not be due to personal peculiarities or to the weather. In Paris and Rome the weather was unfavorable.

Mr. A. Quetelet, at Brussels, locates the radiant at R.A. 148°, N. Dec. 24°. The time of the maximum was, he thinks, about a quarter past one o'clock.

(*k.*) *In Great Britain.*—At Edinburgh one observer saw the following numbers in successive minutes from 12^h 58^m to 1^h 38^m (G. m. time),

* Comptes Rendus, lxiii, 961. Compare this vol., p. 88, No. 25.

another person keeping the record. The sums of the columns give the numbers for each five minutes.

28	30	38	40	44	33
30	36	29	35	31	30
32	34	30	35	30	23
60	64	40	52	29	23
60	38	41	50	29	23
210	202	178	212	163	132

During the next half hour were seen 395, making a total of 1492 for one observer in the hour. Similar records of numbers by other observers are given in the Astr. Soc. Notices.*

Sir John Herschel located the radiant at long. 142° 20′, lat. 10° 15′ N. Since this point is very nearly—perhaps exactly—90° from the sun, he concludes that the orbit of the meteors is very nearly circular and the motion retrograde. He adds (Astr. Soc. Notices, xxvii, 21), "How far this conclusion of a retrograde motion of the meteorites' revolution round the sun—a conclusion already, I believe, arrived at by Mr. Newton—is compatible with the truth of the 'nebular hypothesis,' we may leave it to the advocates of that hypothesis to consider."

A train observed by him remained visible six minutes, drifting slowly *southwards* over a space 8° or 9°, and at the same time changing its direction so that at its disappearance it was at right angles to its original position.

Mr. A. S. Herschel gives (Astron. Soc. Notices) the following determinations of the radiant. Most of them are from paths recorded by different observers upon charts of the British Association.

Long.	N. Lat.	Long.	N. Lat.	Long.	N. Lat.
141° 6′	10° 5′	144° 45′	10° 4′	145° 40′	11° 52′
142 41	10 58	141 37	11 19	143 41	9 54·5
143 7	9 16	143 31	9 28	143 12	10 3
142 10	10 15	141 36	9 27	142 58	9 9
142 28	9 49	146 41	10 20	142 51	10 42

The magnets at Greenwich were remarkably quiet during the night. Prof. Challis remarked an unusual glow in the atmosphere during the display.

(*l.*) *Observations with the spectroscope.*—Mr. A. S. Herschel and Mr. John Browning were provided with spectroscopes arranged for direct vision. Mr. Browning says (Astron. Soc. Notices, 1867, p. 77):

"After catching a few spectra in different directions, I at length decided on keeping the direct-vision prism pointed a little to the west of Leo Major, with the axis of the prism parallel to the horizon. The spectra which I saw were those of meteors which started from the radiant point and passed through the belt of Orion. Of course the number of meteors which came into my field was comparatively limited, but the whole of them travelled in a direction parallel to the axis of the prism, a condition essential in the observation of the spectra.

"From the rapid flight of the meteors rendering the spectra very difficult to catch, I cannot pretend to speak with confidence of the appear-

* There is an important misprint in the 25th line, p. 87 of the last number of the Journal, in Dr. Gould's letter. For 12h 39m 0s, read 12h 59m 0s.

ance of the spectra shown by the prism, but I saw a great difference between the spectra. I believe that I saw spectra of the following kinds:

"A. Continuous spectra, or those in which the whole of the colors of the solar spectrum were visible, excepting the violet rays.

"B. Spectra in which the yellow greatly preponderated; but which in every other respect resembled those above described.

"C. Spectra of almost purely homogeneous yellow light, but with a faint continuous spectrum, that is, a faint trace of red on one side and green on the opposite side of the yellow portion of the spectrum.

"D. Spectra of purely homogeneous green light; of this kind I only saw two.

"I observed through the prism spectra of several trains. The light which was mostly blue, green, or steel gray, generally appeared homogeneous; but this may have arisen from the light having been too faint to produce a visible spectrum. Stars below the second or third magnitude, although visible through the prism, fail from this cause to give spectra in which blue and red are perceptible."

As was indicated in the last number of the Journal (p. 88), the eastern limit of the shower must have been in Central Asia. It was a little east, however, of the line there given.

Several papers concerning the theory of the meteors by Schiaparelli, Faye, LeVerrier, Peters, &c., of which we had intended to give here an abstract, must be deferred to the next number of the Journal. H. A. N.

2. *New minor planet, Antiope,* (90).—Dr. Luther discovered a minor planet on the 1st of October, to which the name Antiope has been given.

3. *New minor planet,* (91).—The ninety-first minor planet was discovered by Mr. Stephan at Marseilles on the 4th of November.

4. *Comet.*—Mr. Stephan discovered a telescopic comet on the night of the 22d of January, in R.A. $2^h\ 34^m$, and N.P.D. 74° 26′.

5. *Aurora Borealis at Highland, Illinois;* by A. F. Bandelier. (From a letter to the editors, dated Highland, Madison Co., Ill., Nov. 11, 1866.) —I find in No. 119, 2d series of this Journal, (Sept. 1865,) observations on the aurora of Aug. 3, 1865, at your city, among which I notice some remarks on the change of color of auroral streamers, from white and yellow into a rosy hue; which the observer attributes to the effect of "*sunlight* striking the tops of those streamers at the height of several hundred miles—as it must have been at that hour—above the earth's surface." The learned author of the said communication calling the attention of auroral observers to this fact, I venture to transmit to him, through your kind intermediary, some extracts from my auroral note-book (kept since 1860, and containing new observations of 47 displays), bearing on the points alluded to by him.

1860, Aug. 12, 9·4 P. M.—A group of splendid streamers appeared N. 15° W., white at first, but turning into purple above.

1860, Sept. 6, 8·33 P. M.—A bluish glow, N. 30° W., issuing a cluster of red streamers; they shifted slowly toward Ursa Major, color turning from purple into an intensive bloody red.

10·5.—The entire upper border of the dark segment dissolved into a smoky cumulated mass, out of which a perfect sea of streamers is seen to issue. The streamers are white below, purple above, the most vivid red being always toward the middle of the streamer.

10·25.—E. and W. the streamers incline toward the horizon, and assume the deepest red hue, while in the N. the color is much paler.

1861, March 9, 8·20 P. M.—The dark segment has completely vanished and a semicircle of streamers has taken its place. They reach as high as the pole-star, and assume a red hue above an altitude of 25°. The most beautiful color is at both the eastern and western extremities.

Observation made on Durham Terrace, Quebec, Aug. 7, 1862, in bright moonlight. Yellow streamers rose from N.E. to N.W., their base being at an altitude of 25°–28° and the tops reaching the zenith. . . . . These streamers appeared stationary, but the intensity of the light moved regularly through their feet from E. to W. and backward, also flowing upward along the streamer. (Same observation at 8·10 P. M. of Aug. 8, on St. Lawrence river, near Trois-Rivières, moon shining very brightly. Streamers of yellow hue.)

1862, Oct. 3.—Full moonlight and display rather indistinct on account of it. I noticed the sky to turn purple in the E. at a low altitude, and at the beginning of the aurora.

Feb. 20.—Near the moon there was a patch of pale carmine, of alternately increasing and decreasing intensity.

In general, I have never seen a single streamer that was not originally *white*, but turned sometimes into purple or bloody-red *upon reaching a certain altitude*. At the moment of issuing, the beam is white and brightest at its base; as it increases in size, it generally attains a motion along the horizon to the west, and also the intensity decreases below and appears greatest in the middle of the beam. When the beam has reached its most westerly position, then the base vanishes and the top appears brightest, the streamer appears as an isolated cloud of more or less brilliant light of varying intensity, until it begins to vanish.

When several arches or layers of auroral matter succeed each other and the tops of the streamers of one arch (still below our horizon) appear at their vanishing points behind the arch immediately preceding, we may sometimes notice the sky under an auroral arch in sight to turn purple also.

I have also noticed that the condition of the atmosphere has great influence on the color of auroral light. By hazy weather streamers appear red, nearer the horizon than by a clear transparent sky.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Analysis of a Meteoric Iron from Colorado;* by Dr. C. T. Jackson. (From a letter to one of the editors, dated Boston, Nov. 10, 1866.)—I received last Tuesday, Nov. 6th, a piece of meteoric iron from Rev. Mr. Thompson, who brought it from Colorado and who had negotiated for the large mass with the intention of presenting it to the Boston Society of Natural History. I have just learned that Prof. Shepard through the agency of a friend in Denver City has secured the original mass, said to be two feet in diameter, for his cabinet. It appears from Mr. Shepard's letter to me that it is the same mass that is mentioned in the last (Sept.) No. of your Journal, page 250. I made the chemical analysis of it before being aware it was the same meteorite described, and since no previ-

ous analysis of it has been made, I offer mine to you for the Journal, Prof. Shepard expressing a desire that it should be published.

The piece of meteoric iron given me by Mr. Thompson, who brought it from Colorado, weighs four ounces. It has been heated in a forge fire in order to cut it more easily; but still the Widmannstättian figures come out, when dilute nitric acid is applied to the polished surface, as distinctly as possible and consist of a series of small nearly equilateral triangles with the lines well defined and quite elevated. On one side of the specimen was a crust about one-eighth of an inch thick, consisting of sulphid of iron. This probably in the unaltered meteorite is a bisulphid of iron mixed with oxyd of iron.

A portion of the clean metal sawed off from the mass has the sp. gr.= 7·692.

On chemical analysis by the most approved method, separating the iron from the nickel by succinate of ammonia and determining the nickel as oxyd of nickel and then analyzing this oxyd for cobalt and copper—a separate portion of the meteorite being employed in analysis for the tin which was twice determined, and the nitric solution being tested for phosphoric acid and sulphuric acid, &c.—the results per cent of my analysis are as follows:

Metallic iron,	90·650
" nickel,	7·867
" cobalt,	0·010
" tin,	0·020
Insoluble matter consisting of a little silica, schreibersite and chrome as proved by blowpipe investigation,	0·950
	99·497

2. *Hailstones in China;* by S. W. Williams.—On Tuesday at 6 P. M. on June 5, 1866, a thunder storm came from the northeast, and broke over Peking with great violence. The hailstones soon followed the first dash of rain, and increased in size and quantity till the rain almost seemed to cease. The shower lasted forty minutes, leaving the yards white with hailstones, but as the wind was light no damage was done. The very largest stones were 4 to 4½ inches in circumference; the prevailing shape was conical, and almost all the stones exhibited a kernel of clear ice enclosed in frozen snow, with a covering of ice outside. The strata of air through which they passed in their descent must have been of very different degrees of temperature to produce such distinct layers of ice and snow in the stones. Such hailstorms are not frequent in the North of China, and the people say that this one is the most remarkable since July, 1838, when the stones were like oranges and apples and melons for size, and did great damage to dwellings and trees.

3. *U. S. Coast Survey.*—The eminent mathematician, Prof. Pierce of Harvard, has been appointed to the office of Superintendent of the Coast Survey, left vacant by the death of Prof. Bache.

4. *Chicago Museum of Natural History.*—The late Major Kennicutt at the time of his death was Director of the Chicago Museum of Natural History, an institution of which he was essentially the founder. This office has recently been filled by the appointment of Mr. William Stimpson, one of the best zoologists of the country.

OBITUARY.

Prof. BACHE.—The death of Professor ALEXANDER DALLAS BACHE, just announced, will awaken profound regret throughout a very large circle of scientific friends. For the last thirty years he has been intimately connected with the progress of American Science. In some important departments he, more than any other man, may be regarded as the leader.

He was graduated at the Military Academy at West Point, in 1825, holding the first rank in his class. He was immediately appointed Assistant Professor of Engineering in the Academy, and occupied the position for one year. After serving as an officer in the Corps of Engineers for three years, he resigned to accept the professorship of Natural Philosophy and Chemistry in the University of Pennsylvania, to which he was elected in 1827. After filling this place with distinguished success, for a number of years, he was called, in 1836, to the presidency of the Girard College, then recently established in Philadelphia by the princely bequest of Stephen Girard. Six years later he received and accepted the appointment of Superintendent of the United States Coast Survey, a post made vacant by the death of Professor Hassler. He entered upon this office in November, 1843. Since that time, a period of twenty-three years, the results of his labors have been public property. It would be out of place in this brief notice to speak of the magnitude or the importance of that great national work, the Coast Survey. It is proper to say, however, that few men could have carried to it such ample scientific preparation, so much practical wisdom, and such signal, almost unrivalled, administrative talents. His annual reports to Congress, growing in fulness and extent as the work advanced, form an invaluable series of scientific papers. They have justly won for him not only an American but a European reputation. It is well known that eminent scientists in Europe, engaged in the vast labors of geodesy undertaken by the different governments, have sought with eagerness for the Reports of the American Coast Survey, and have placed them, as regards accuracy and exhaustive thoroughness, in the first rank of works upon that subject.

He has contributed many memoirs to our scientific journals, and many to the American Association for the Advancement of Science, of which he was twice president, and always a leading member. These memoirs, often very elaborate, have generally been devoted to the discussion of original researches in the more progressive branches of physical inquiry, and are well worthy of attention not only as substantial contributions to science, but as models of research.

During his presidency of Girard College from 1836 to 1841, he spent a year abroad under the direction of the Board of Trustees, to examine and report upon the state of education in Europe. The results of this examination, executed with great care and minuteness of detail, were given to the public in a full and very instructive octavo volume in 1839.

On the establishment of the National Academy of Sciences, by the Act of Congress, in 1863, the members of that body, intended to represent and direct the highest science of the country, unanimously elected him their first president for a period of six years. Unhappily for the interests of science that period has been cut short. The disease which has now terminated fatally, induced perhaps by over mental action, has for many months been making inroads upon his fine physical constitution, and

impairing the vigor of his large and well-balanced powers of mind. His friends have watched its progress with alternate hopes and fears, and have only recently yielded to the sad conviction that his allotted work was done.

In the administration of his office as Superintendent of the Coast Survey, Prof. Bache was always kind and considerate to his subordinates —but never blind to any remissness in duty. He was himself a great worker and he expected every body under him to follow his example.

In society he was eminently genial. No one knew better than he, how to throw off the care of business when a task was done, and give himself up to the mirth and merriment of the hour. To his friends he was most generous and obliging, and very many of them will feel his death as a sorrowful, personal bereavement. He leaves a devoted wife, who has been many years literally the sharer of his labors, to mourn her irreparable loss and bear the burden of solitude in the midst of society. A. C.

J. Burkhardt.—Mr. Burkhardt, long associated with Prof. Agassiz as his artist, died on the 20th of February last, from the effects of a disease consequent on exposure in the course of the late Brazilian expedition.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *First Annual Report on the Geology of Kansas;* by B. F. Mudge, A.M., Prof. of Geology and Nat. Hist. in the Kansas State Agricult. College, and State Geologist for 1864. Lawrence, 1866. 56 pp., 8vo.

Preliminary Report of the Geological Survey of Kansas; by G. C. Swallow, State Geologist. Lawrence, 1866. 198 pp., 8vo.

The first of these Reports gives the results of a reconnoissance made in 1864. It contains some facts of general interest to the people of Kansas, —especially in regard to the saline springs, and the manufacture of salt, but nothing new to science.

The second Report is a more important work. It contains the results of an examination of Eastern and Central Kansas, made in 1865, and includes the separate Reports of Dr. C. A. Logan on the sanitary relations of the State, that of Dr. T. Sinks on its Climatology, and the Report of the Assistant Geologist, Major F. Hawn.

A detailed section of the rocks of Eastern Kansas is first given, in which the classification of the Permian proposed by Swallow and Hawn is essentially retained. This makes the formation in that region about 700 feet in thickness, or nearly three times that admitted by Meek and Hayden (this Journal, [2], xxvii, 424). These geologists regarded the intermediate strata, which contain both Permian and Coal-measure fossils, as part of the Upper Coal-measures, resting conformably upon those below. In this Report these strata are considered as Permian; and it is claimed by Prof. Swallow that a want of conformability may be detected between them and the true Coal-measures, by comparing sections made at different localities. This question of conformability in the series is a very important one, and deserves further investigation.

According to this Report the oldest rocks in the State are Lower Carboniferous. The Coal-measures occupy the surface of Eastern Kansas over an area of 17,000 square miles, and dip beneath the Permian to

the westward. They contain not less than 80 separate beds of limestone. No additional evidence appears to have been obtained in regard to the age of the gypsum-bearing strata immediately above the Permian, and none relating to the more recent formations.

The report contains a chapter on economical geology, which, with the separate Reports appended, furnishes much information of value to all interested in Kansas.

2. *First Annual Report of the Geological Survey of Iowa;* by C. A. WHITE, M.D., State Geologist. 4 pp., 8vo. Des Moines, 1867.—This Report is merely a preliminary notice of the organization and commencement of the present survey during the past year. A brief Report by the State Chemist, Prof. Hinrichs, is appended.

3. *Report of the Progress of the Geological Survey of North Carolina*, 1866; by Prof. W. C. KERR, State Geologist. 56 pp., 8vo. Raleigh, 1867.—A brief Report, containing some information on the geology of the State, but mainly interesting as showing that its geological exploration is begun again in good earnest.

4. *Geological Survey of Canada*, Sir WM. E. LOGAN, Director. Report of Progress from 1863 to 1866. 322 pp., large 8vo. Ottawa, 1866.—This Report reached us too late for a notice in this place.

5. *On the Rock-Salt Deposit of Petit Anse, Louisiana Rock-Salt Company.* Report of the American Bureau of Mines. 36 pp., 4to, with maps.—This important report is based mainly on the investigations of Dr. C. A. Goessmann. Dr. Goessmann obtained for the composition of the salt of Petit Anse, chlorid of sodium 98·8823, sulphate of lime 0·7825, chlorid of magnesium 0·0030, chl. of calcium 0·0036, moisture 0·3286=100.

6. *Plane Problems in Elementary Geometry: or Problems on the Elementary Conic Sections, the Point, Straight line, and Circle;* by S. EDWARD WARREN, C.E., Prof. of Descriptive Geometry, etc., in the Rensselaer Polytechnic Institute. 162 pp., 12mo, with a plate and numerous figures. New York, 1867 (John Wiley & Son).—This little volume is prepared by one who is master of his subject both theoretically and practically, and is an excellent manual for the student or artizan. It gives directions with regard to the use of drawing instruments, is clear and precise in its definitions and demonstrations, and very varied in its problems.

7. *The American Naturalist, a Popular illustrated Magazine of Natural History.* Vol. I, March, 1867, No. 1. 56 pp., 8vo. Salem, Essex Institute.—This first number of the monthly American Naturalist, announced in this volume, at page 136, sustains fully all that was promised. Among its illustrations are two plates, one, of the crater of Kilauea in 1864–5, and the other, of the structure of the Land Snails, with reference especially to those of New England.

8. *Description of Fossil Plants from the Chinese Coal-bearing rocks;* by J. S. NEWBERRY, M.D., being Appendix No. 1 of Geological Researches in China, Mongolia and Japan, by Raphael Pumpelly. 5 pp., 8vo, with a plate. Smithsonian Contrib. to Knowledge, 1867.—The species are those referred to by Dr. Newberry in his paper in this Journal, vol. xlii, p. 151.

9. *Musée Teyler:* Catalogue systématique de la Collection Paléontologique, par T. C. WINKLER. Quatrième Livraison. Harlem, 1865.—This is a continuation of the Catalogue noticed in vol. xli, at page 287.